TRANSACTIONS OF THE

INSTITUTION OF NAVAL ARCHITECTS.

1883.

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Naval Constructor

TRANSACTIONS

OF THE

INSTITUTION OF NAVAL ARCHITECTS.

VOLUME XXIV.

EDITED BY GEORGE HOLMES,

SECRETARY OF THE INSTITUTION.

1883.

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The objects of the Institution of Naval Architects—which was established to promote the Improvement of Ships, and of all that specially appertains to them—are comprised under three heads :—

First, the bringing together of those results of experience which so many shipbuilders, marine engineers, naval officers, yachtsmen, and others acquire, independently of each other, in various parts of the country, and which, though almost valueless when unconnected, doubtless tend much to improve our Navies when brought together in the printed Transactions of an Institution.

Secondly, the carrying out, by the collective agency of the Institution, of such experimental and other inquiries as may be deemed essential to the promotion of the science and art of shipbuilding, but are of too great magnitude for private persons to undertake individually.

Thirdly, the examination of new inventions, and the investigation of those professional questions which often arise, and were left undecided before the establishment of this Institution, because no public body to which professional reference could be made then existed.

BYE-LAWS AND REGULATIONS.

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1. The Institution of Naval Architects shall consist of four classes, *viz.*, Members, Associates, Honorary Members, and Honorary Associates.

2. *Members.*—The Class of Members shall consist exclusively of Naval Architects, and Marine Engineers conversant with Naval Architecture.

3. *Associates.*—The Class of Associates shall consist of persons who are qualified, either by profession or occupation, or by scientific or other attainments, to discuss with Naval Architects the qualities of a ship, or the construction, manufacture, or arrangement of some part or parts of a ship or her equipment.

4. *Honorary Members.*—The Class of Honorary Members shall consist of persons who are eligible as Members, and upon whom the Council may see fit to confer an honorary distinction.

5. *Honorary Associates.*—The Class of Honorary Associates shall consist of persons who have contributed to the improvement of ships or their equipment, and upon whom the Council may see fit to confer an honorary distinction.

ELECTION AND DUTIES OF OFFICERS.

6. The Officers of the Institution shall consist of a President, Vice-Presidents, Members of Council, Associate Members of Council (not exceeding in number one third the number of Members of Council), a Treasurer, two Auditors of Accounts, and a Secretary or Secretaries.

7. A General Meeting of the Members and Associates of the Institution shall be held annually before Easter in each year ; and at this Annual General Meeting the Members of Council, Associate Members of Council, Treasurer, and Auditors for the ensuing year shall be elected.

8. At the Annual General Meeting Members only shall vote in the Election of Members of Council, and both Associates and Members in the election of Associate Members of Council, the Treasurer, and the Auditors.

9. As soon as may be convenient after the Annual General Meeting, the newly-elected Members and Associate Members of Council shall meet and elect the President and Vice-Presidents for the year, Members and Associate Members of Council both voting.

10. *President.*—Both Members and Associates of the Institution shall be eligible for election as President. The President shall preside over all meetings of the Institution, and of Officers of the Institution, at which he is present, and shall regulate and keep order in the proceedings.

11. *Vice-Presidents.*—Both Members and Associates of the Institution shall be eligible for election as Vice-Presidents. In the absence of the President, one of the Vice-Presidents shall preside at the General Meetings of the Institution, and shall regulate and keep order in the proceedings.

12. In case of the absence of the President and of all the Vice-Presidents, the Meeting may elect any Member of Council or Associate Member of Council, and in case of their absence any Member present, to preside.

13. The Chairman at any Meeting of the Council of the Institution, when the votes of the meeting, including his own, are equally divided, shall be entitled to give a casting vote.

14. Persons holding the office of Vice-President shall at all times be entitled to sit and vote with the Council.

15. *Past Presidents and Vice-Presidents.*—All Members who have held the posts of President and Vice-President shall, while their connection with the Institution as Members lasts, be entitled to sit and vote with the Members of Council.

16. *Members of Council.*—Members only shall be eligible for Election as Members of Council at the Annual General Meeting.

17. *Associate Members of Council.*—Associates only shall be eligible for Election as Associate Members of Council at the Annual General Meeting.

18. The Direction and Management of the Institution shall be vested in the Council for the time being, the Associate Members voting with the Members of Council in all cases, except in the decision of questions directly affecting the forms of ships and the construction of their hulls.

19. The Council shall meet as often as the business of the Institution requires, and at every Meeting five Members of the Council shall form a *quorum.*

20. The Council may appoint Committees to report to them upon special subjects.

21. All questions shall be decided in the Council by vote; but at the desire expressed in writing, of any four Members or Associate Members present, the determination of any subject shall be postponed to the succeeding Meeting of the Council.

22. An annual statement of the funds of the Institution, and of the receipts and payments of the past year, shall be made under the direction of the Council, and after having been verified and signed by the Auditors, shall be laid before the Annual General Meeting.

23. The Council shall draw up an Annual Report on the state of the Institution, which shall be read at the Annual General Meeting.

24. It shall be the duty of the Council to adopt every possible means of advancing the Institution, to provide for properly conducting its business in all cases of emergency, such as the death or resignation of Officers, and to arrange for the publication of the Papers read at the Meetings, or of such documents as may be calculated to advance the objects of the Institution.

25. *Treasurer.*—Only Bankers, or Members of Council, or persons who have been Members of Council and are still Members of the Institution, shall be eligible for election as Treasurer.

26. *Auditors.*—All Members and Associates of the Institution shall be eligible for election as Auditors.

27. The Auditors shall have access at all reasonable times to the Accounts of the pecuniary transactions of the Institution; and they shall examine and sign the annual statement of the Accounts before it is submitted by the Council to the Annual General Meeting.

28. *Secretary.*—The Secretary or Secretaries shall be elected by the Council, and shall be removable at the will of the Council, after due notice given. The salary of the Secretary or Secretaries shall be fixed by the Council.

29. It shall be the duty of the Secretary, under the direction of the Council, to conduct the correspondence of the Institution; to attend all meetings of the Institution and of the Council; to take Minutes of the proceedings of such Meetings; to read the Minutes of the preceding Meeting; to announce donations made to the Institution; to superintend the publication of such Papers as the Council may direct; to have charge of the library, museum, and offices of the Institution; and to direct the collection of subscriptions and the preparation of accounts. He shall also engage, and be responsible for, all persons employed under him, and generally conduct the ordinary business of the Institution.

30. In January of each year the Council shall meet and prepare Lists for the election of the Council for the ensuing year. These Lists shall be as follows, namely:—

1st. A List of the names of the President, Vice-Presidents, and Treasurer for the ensuing year to be submitted at the Annual General Meeting, for the election in a body.

2nd. Lists for the election of the Ordinary Members and Associate Members of Council. This List shall contain the names of the existing Ordinary Members of Council and Associate Members of Council, and at least eight new names, *six* of whom shall be of Members, and at least *two* of Associates of the Institution.

31. No addition shall be made to the total number of Ordinary Members of the Council until, by death or resignation, their numbers shall have been reduced below twenty-four, after which their numbers shall be raised to and preserved at twenty-four. And no addition shall be made to the Associate Members of Council until, by death or resignation, their numbers shall have been reduced below eight, after which their numbers shall be raised to and preserved at eight, always exclusive of the President, Vice-Presidents, and Treasurer.

32. At the date of issuing the Syllabus of the Annual General Meetings in each year, the complete Lists proposed by the Council for the Ordinary Council for the ensuing year shall be printed, and sent to all Members to serve as Balloting Lists. These Lists shall contain, first, the names of the existing Ordinary Members of Council at the time of the preparation of the Balloting List, together with six new names of Members of the Institution (or, if the number shall have been reduced below twenty-four, then so many new names as shall be needed to bring the number up to thirty), and the whole of these names shall be printed in alphabetical order. Secondly, the names of the existing Associate Members of Council at the time of the preparation of the Balloting List, together with two new names of Associates of the Institution (or, if the number shall have been reduced below eight, then so many new names as shall be needed to bring the number up to ten), and these names also shall be printed in alphabetical order. From these Lists the new Council shall be elected. Every Member shall be at liberty to strike out any name on the Lists, but he must strike out *six* names from the List of Members of Council, and *two* names from the List of Associate Members of Council.

33. A similar Balloting List (in which, however, the names of the ordinary Members of Council proposed for Election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall be at liberty to strike out any names on that List; but he *must* reduce the number by striking out *two* names from the List of Associate Members of Council.

34. The Balloting Lists may be sent by post or otherwise to the Secretary, so as to reach him before the day and hour named for the Annual General Meeting, or they may be personally presented by the Members and Associates at the opening of the Annual General Meeting.

35. At the opening of the Annual General Meeting the order of business shall be :—

(1.) To read and consider the Reports of the Council and Treasurer.

(2.) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3.) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents, and Treasurer for election for the ensuing year.

(4.) The Chairman shall next nominate two Scrutineers (of whom one only shall be a Member of the existing or proposed Council), and shall hand to them the Ballot Boxes containing the Voting Papers for the Ordinary Members of Council and Associate Members of Council; and

(5.) The Scrutineers, assisted by the Secretary and his clerks, shall receive the Ballot Papers which may have reached him, and all others which may be presented by Members or Associates at the Meeting. The Scrutineers shall then retire, and verify the Lists, and count the votes; and shall, not later than the following day, report to the Chairman the names which have obtained the greatest number of votes, subject to the conditions of the Ballot. The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected.

(6.) After the Ballot shall have been taken, and the Scrutineers have retired, the Meeting will proceed to the other business before it.

36. The new Council and Officers shall take office immediately after the close of the Annual General Meeting.

DESIGNATION OF MEMBERS AND ASSOCIATES.

37. Any Member, Associate, Honorary Member, or Honorary Associate, having occasion to designate himself as belonging to the Institution, shall state the class to which he belongs according to the following abbreviated forms, viz., M.I.N.A.; Assoc. I.N.A.; Hon. Mem. I.N.A.; Hon. Assoc. I.N.A.

ELECTION OF MEMBERS AND ASSOCIATES.

38. *Admission of Members.*—Every Candidate for admission into the Class of Members, or for transfer into that Class from the Class of Associates, shall be more than twenty-five years of age, and shall comply with the following Regulation :—

He shall submit to the Council a statement showing that he has been professionally engaged in shipbuilding or marine engineering for at least seven years in some public or private shipbuilding establishment, or marine engine works, and setting forth the grounds upon which he bases his claims to be considered a professional Naval Architect or Marine Engineer, and to be admitted as such to the Membership of the Institution. This shall be signed by at least three Members whose signatures shall certify their personal knowledge of the Candidate, and approval of his statement; or, in the case of persons not British born, the signatures of three Members shall be required, in confirmation of their personal knowledge of the Candidate's scientific reputation.

39. These preliminary conditions being satisfied, the Council shall then consider whether the practical experience and professional attainments of the Candidate are such as entitle him to be brought forward by the Council as a Naval Architect, or Marine Engineer conversant with Naval Architecture. If four-fifths at least of the received votes of the professional Members of the Council are in favour of his application, and such four-fifths constitute a majority of the professional Members of the Council, his proposal for admission shall be submitted to the Members of the Institution (who shall have access to the applicant's statement), at an Ordinary Meeting of the Institution, for them to vote upon, by ballot.

40. *Admission of Associates.*—Candidates for Associateship shall submit to the Council a proposal for their admission, setting forth therein a statement of their claims to be admitted as Associates. Their proposal, if approved by the Council, shall be submitted by them at an Ordinary Meeting of the Institution, for the Members and Associates jointly to vote upon by ballot.

41. The proportion of votes for deciding the election of Members and Associates shall be at least four-fifths of the numbers recorded.

SUBSCRIPTIONS.

42. Each Member and Associate shall pay an Entrance Fee of two guineas, and an Annual Subscription of two guineas in advance; the first subscription being payable on his election, and all future ones on the 1st day of January of each year. Any Member or Associate withdrawing from the Institution after that date is still liable for the amount of Subscription due on that day.

43. Any Member or Associate may compound for his Annual Subscription, for life, by a single payment of not less than thirty guineas.

44. No person's name shall be entered on the Roll as Member or Associate of the Institution, nor possess the privileges of Membership (except it be on the honorary list), until he shall have paid his first subscription or the life-composition, and if the payment be delayed for more than twelve months from the date of his election, the same shall be void, unless the Council otherwise direct.

45. The Secretary shall at the close of every year notify to all Members and Associates whose subscription for that year shall not have been paid, that it will be his duty to report accordingly to the Council, and he shall at the same time furnish the person whose subscription is in arrear with copies of this and of the two following Rules.

46. The Secretary shall before Easter in every year lay before the Council a list of all Members and Associates whose subscriptions for the two previous years shall be still unpaid, and, unless the Council shall otherwise direct, the names of those in arrear shall be expunged from the Roll of Members and Associates, and shall not be replaced without re-election in due form. Provided always that the Council shall at any time within two years therefrom have power to dispense with such re-election, and to restore the name to the Roll upon payment of all subscriptions then due, and upon cause being shown to the satisfaction of the Council why such subscriptions were not previously paid.

47. Nothing herein contained shall prejudice the right of the Institution to the legal recovering of all arrears of subscriptions up to the date of striking the name off the Roll.

48. In case the Council should be of opinion that any Member, who has been long distinguished in his professional career, from ill-health, advanced age, or other sufficient causes, should not be called upon to continue his annual subscription, they may remit it. Also they may remit any arrears which are due from an individual, or may accept a collection of books, or drawings, or models, or other such contribution as, in their opinion, under the circumstances of the case, may entitle the person to be enrolled as a Life Subscriber, or to enable him to resume his former rank in the Institution which may have been in abeyance from any particular causes. These cases must be considered and reported upon by a Sub-Committee named for the purpose.

49. In case the expulsion of any individual shall be judged expedient by ten or more Members, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary shall be by him laid before the Council for consideration. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in any Minutes nor shall any public discussion thereon be permitted; but if the Council do find good reason for the proposed expulsion they shall direct the Secretary to address a letter to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes nor any public discussion on the subject shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members and Associates, for the purpose of deciding on the question of expulsion; and if two-thirds of the

persons present at such Special General Meeting, providing the number so present be not less than thirty, vote that such individual be expelled, the Chairman of that Meeting shall declare such expulsion accordingly, and the Secretary shall communicate the same to the individual.

MEETINGS.

50. Meetings for the Reading of Papers shall be held as frequently, and at such times, as the Council may determine.

TRANSACTIONS.

51. The *Transactions* of the Institution, including the Papers read at the Ordinary Meetings, and Reports of the Discussions by which they are followed, shall be edited by the Secretary, and printed under the direction of the Council.

52. A copy of each Volume of *Transactions* shall be sent free to every Member and Associate.

53. The Secretary, under the direction of the Council, may dispose of the surplus stock of *Transactions* which have been published more than three years, at a price of not less than One Guinea a volume, provided a sufficient number remain on hand to supply the probable demand of new Members and Associates to complete their sets by the purchase of the back Volumes.

CHANGE OF ADDRESS.

54. Members and Associates are particularly requested to communicate to the Secretary any change of address.

INTRODUCTORY PROCEEDINGS.

THE Meetings of this, the Twenty-fourth Session of the Institution of Naval Architects, were held on the 14th, 15th, and 16th March, 1883, in the Hall of the Society of Arts, John-street, Adelphi.

The opening Meeting was presided over by the Right Hon. Earl RAVENSWORTH, President of the Institution, who commenced the proceedings by calling upon the Secretary, Mr. GEORGE HOLMES, to read the Report of the Council, which was as follows:—

REPORT OF COUNCIL, 1883.

"The Council is glad to be able to report that the finances of the Institution are in a satisfactory state. As will be seen from the annexed balance-sheet, the balance in hand on general account at the end of the year 1882, viz., £338 16s., was smaller than at the corresponding period of the previous year. The diminution was due to an exceptional cause, viz., the large expenditure of £253 18s. 2d. on the printing of the General Index of the first twenty-one volumes of the Transactions. This item is of an altogether exceptional nature, and the Council is able to state that the estimates of expenditure for the current year show that in all probability the balance will be more than restored to its previous figure by the end of the year. On the other hand, the balance in hand to the credit of the Library Fund, viz., £279 19s. 3d., shows a satisfactory increase.

"The Council deeply regrets having to announce the death of an esteemed colleague, the late Mr. John Scott Russell. Mr. Scott Russell's great interest and active participation in the affairs of the Institution were well known to all Members. He was associated with the Institution from the time of its foundation. The Council trusts that it has met the wishes of the Members generally by publishing his portrait in the last volume of the Transactions. A record of his services to the Institution will be found in the Obituary Notice in the same volume.

"During the past year two Exhibitions of an interesting character have taken place. The first, promoted by the Shipwrights' Company, was held in the Fishmongers' Hall in May, and brought together a large collection of ship's models, not merely from British shipyards, but also from abroad. The exhibits from France, Holland, and Norway and Sweden were of a special interest.

"During the autumn an Exhibition of Naval Architecture and Marine Engineering was held at Tynemouth, Newcastle-on-Tyne. The Committee of the Exhibition having invited a deputation from the Council to be present at the opening, the following gentlemen kindly consented to represent the Institution on that occasion, viz., Messrs. W. John, A. C. Kirk, W. Parker, and W. H. White. The Exhibition, which was opened on the 6th of last September by the Earl of Ravensworth, proved to be a very complete and interesting collection of ships' models, marine engines, ships' fittings, and objects intimately connected with naval architecture and the maritime trade of the district. Many of the models were contributed by firms from the principal shipbuilding ports of the United Kingdom. It afforded a practical proof of the great development of the shipbuilding industry in the North-Eastern ports.

"The Council much regrets that it has during a great part of the year been deprived of the co-operation of Mr. Merrifield, in consequence of his illness. Mr. Merrifield's long and constant services to this Institution are well known to the Members. The Council has already elected him an Honorary Associate, and it now welcomes the opportunity of conferring upon him a mark of appreciation and gratitude by proposing to the General Meeting that his name be added to the list of Vice-Presidents.

"The new syllabus for the examination of students in Naval Architecture by the Science and Art Department, which was adopted in consequence of the action of this Institution in the year 1881, is now in practical operation throughout the country, and it is believed that it will accomplish the objects which were then had in view.

"The Council is enabled to report that the popularity of the Institution and the interest taken in its proceedings continue to increase, as is evidenced by the number and character of the papers which are read and discussed at the annual meetings. In order better to meet the convenience of those members who reside at a distance, it has been determined for the future to post to all Members and Associates immediately after the meetings copies of all the papers read, in lieu of the newspaper abstract which has now for some years been published. By this arrangement members will be enabled to obtain copies of the papers while interest in the proceedings is still fresh. It will also enable the volume of Transactions to appear at a much earlier date than usual."

Institution of Naval Architects.

Statement of Income and Expenditure for the Year 1882.

Dr.

		£ s. d.	£ s. d.
1881.			
Dec. 30th	To Balance:—		
	In Treasurer's hands ...	388 11 11	
	In Secretary's hands ...	72 4 0	
			460 15 11
1882.			
Dec. 31st.	Annual Subscriptions ...		1,057 10 11
	Sale of Volume		128 15 0
	Admiralty Grant		250 0 0
			£1,897 1 10

Cr.

		£ s. d.	£ s. d.
1882.			
Dec. 31st.	By Cash Paid:—		
	Rent of Rooms		200 0 0
	Cleaning Rooms		37 14 0
	Attendance at Meetings ...		16 12 6
	Reporting at Meetings ...		45 0 0
	Abstract of Proceedings ...		37 10 0
	Advertising		45 19 4
	Printing Vol. XXII. Engraving " Binding "	391 18 0	
	Miscellaneous Printing ...	63 8 3	
	Printing General Index ...	253 18 2	
	Despatch of Volume ...	32 9 7	
			741 14 0
	Stationery	12 7 6	
	Insurance	2 10 0	
			14 17 6
	Furniture and Repairs ...		15 7 6
	Banker's Charges		1 5 2
	Secretary's Expenses to Newcastle		6 13 6
	Petty Disbursements ...		34 18 4
	Secretary's Salary	287 10 0	
	Clerk's Salary	66 10 0	
	Extra Attendance	6 14 0	
			360 14 0
	Balance:—		
	In Treasurer's hands ...	271 12 6	
	In Secretary's hands—		
	Annual Subscriptions ...	36 17 9	
	Sale of Volumes	9 3 6	
	Current Expenses	21 2 3	
			338 16 0
			£1,897 1 10

Library Account for the Year 1882.

		£ s. d.
1881.		
Dec. 30th	To Balance:—	
	In Treasurer's hands	253 13 1
1882.		
Dec. 31st.	Entrance Fees	60 18 0
		£314 11 1

		£ s. d.
Dec. 31st.	By Library Expenses	19 4 4
	By Furniture and Repairs	15 7 6
	Balance:—	
	In Treasurer's hands	279 19 3
		£314 11 1

J. D'A. SAMUDA, *Treasurer.*
GEORGE HOLMES, *Secretary.*

We have examined the above-written entries with the books and vouchers, and find them correct.

JAMES MACKENZIE, } *Auditors.*
JOHN CORRY,

The following List of Donations to the Library was read by the SECRETARY :—

"Proceedings of the Royal Society." *Presented by the Royal Society.*
"Minutes of the Proceedings of the Institution of Civil Engineers," Vols. LXVII., LXVIII., LXIX., and LXX. *Presented by the Institution of Civil Engineers.*
"Proceedings of the Institution of Mechanical Engineers" for 1882. *Presented by the Institution of Mechanical Engineers.*
"Journal of the Iron and Steel Institute" for 1882. *Presented by the Iron and Steel Institute.*
"Journal of the Society of Arts" for 1882. *Presented by the Society of Arts.*
"Transactions of the Institution of Engineers and Shipbuilders in Scotland," Vol. XXV. *Presented by the Institution of Engineers and Shipbuilders in Scotland.*
"Transactions of the Society of Engineers" for 1882. *Presented by the Society of Engineers.*
"Annual Report of the Board of Regents of the Smithsonian Institution" for 1880. *Presented by the Board of Regents.*
"Lloyd's Register of British and Foreign Shipping" for 1882-83. *Presented by the Committee of Lloyd's Register.*
"Lloyd's Register of Yachts" for 1882. *Presented by the Committee of Lloyd's Register.*
"Underwriters' List of Iron Vessels" for 1882-83. *Presented by the Underwriters' Registry of Iron Vessels, Liverpool.*
"The Scientific Proceedings of the Royal Society of Dublin," Vols. I., II., & III. *Presented by the Royal Society of Dublin.*
"North of England Institute of Mining Engineers" Vol. XXXI. *Presented by the North of England Institute.*
"Engineer" for 1882. *Presented by the Proprietors.*
"Engineering" for 1882. *Presented by the Proprietors.*
"Iron" for 1882. *Presented by the Proprietors.*
"Field" for 1882. *Presented by the Proprietors.*
"Army and Navy Gazette" for 1882. *Presented by the Proprietors.*
"Saturday Review" for 1882. *Presented by the Proprietors.*
"English Mechanic" for 1882. *Presented by the Proprietors.*
"Revue Maritime" for 1882. *Presented by the French Ministry of Marine.*
"Wochenschrift des Osterreichischen Ingenieur- und Architekten-Vereines" for 1882. *Presented by the Proprietors.*
"Proceedings of the Royal Society of Victoria," Vol. XVII. *Presented by the Royal Society of Victoria.*
"Marine Engineer" for 1882. *Presented by the Proprietors.*
"The British Navy," Parts I., II., III. By Sir THOMAS BRASSEY, K.C.B., M.P. *Presented by the Author.*
"A Manual of Marine Engineering." By A. E. SEATON. *Presented by the Author.*
"Souvenirs de Marine Conservés." Par l'Amiral PARIS. *Presented by Captain de la Chauvinière.*
"Germanischer Lloyd." *Presented by* ALFRED A. DIETRICH, Esq.

The following gentlemen (having been duly recommended by the Council) were unanimously elected Members of the Institution :—Mr. James W. Claridge, Engineer, London ; Mr. Alfred A. Dietrich, Chief of Bureau of Construction, Imperial Admiralty, Berlin ; Mr. John G. Dobbie, Superintending Engineer to the British India Steam Navigation Company, Limited, Bombay ; Mr. John Francis Hall, Manager to William Jessop & Sons, Limited, Brightside Steel Works, Sheffield ; Mr. Andrew King Hamilton, Surveyor, Underwriters' Registry, Liverpool ; Mr. John Lennox Lumsden, Shipbuilder, Govan, N.B. ; Mr. Hector MacColl, Manager to Messrs. James Jack & Co., Marine Engineers, Liverpool ; Mr. Edwin Lewis Martin, Draughtsman to F. Elgar, Esq., London ; Mr. Henrik van Meerten, Chief Engineer, Dutch Royal Dockyard and Steam Factory, Onrust, Batavia ; Mr. John Henry Morrison, Draughtsman to the Admiralty, London ; Mr. Joseph Newnham, Portsmouth Dockyard ; Mr. Robert James Quelch, Superintending Marine Engineer and Ship Surveyor, London ; Mr. George Henry Stainer, Assistant to Surveyor of Dockyards, Admiralty, London ; Mr. Alexander Taylor, Consulting Engineer, Newcastle-on-Tyne ; Mr. Joseph Lowes Thompson, Junior Partner in the firm of Messrs. J. L. Thompson & Sons, Sunderland ; Mr. Thomas Todd, Superintending Engineer to Messrs. H.

Clarkson & Co., London; Mr. Robert Zimmermann, Chief Draughtsman, Barrow Shipbuilding Company, Barrow-in-Furness.

The SECRETARY next read the following list of names nominated by the Council to serve as Ordinary and Associate Members of Council for the ensuing year:*—Mr. Frederick K. Barnes, Mr. W. J. Bone, Mr. William Denny, Mr. Raylton Dixon, Mr. Theodore Doxford, Mr. Francis Elgar, Mr. Joshua Field, Mr. J. MacFarlane Gray, Mr. Henry Green, Mr. Edward J. Harland, Mr. John Inglis, Junior, Mr. William John, Mr. Alexander C. Kirk, Mr. Henry H. Laird, Mr. Andrew Leslie, Mr. Benjamin Martell, Mr. Henry Morgan, Mr. John Napier, Mr. William Parker, Mr. William Pearce, Mr. John Penn, Mr. John Price, Mr. John R. Ravenhill, Mr. George B. Rennie, Mr. Henry F. Swan, Mr. John I. Thornycroft, Mr. E. C. Warren, Mr. Bernard Waymouth, Mr. H. H. West, Mr. John White, Mr. William H. White, and Mr. C. H. Wigram, as Members of Council; and the Most Hon. the Marquis of Ailsa, Sir F. J. Bramwell, F.R.S., Professor J. H. Cotterill, F.R.S., Mr. E. A. Cowper, Mr. R. E. Froude, Mr. Dixon Kemp, Mr. C. W. Merrifield, F.R.S., Sir James Ramsden, Mr. W. W. Rundell, Dr. C.W. Siemens, F.R.S., D.C.L., and Mr. W. H. Tindall, as Associate Members of Council.

The PRESIDENT (Earl of Ravensworth) next put to the meeting the following list, containing the names of the President and Vice-Presidents proposed for the ensuing year, and which was unanimously adopted:—The Right Hon. the Earl of Ravensworth, President; H.R.H. Vice-Admiral the Duke of Edinburgh, K.G.; the Right Hon. the Earl of Northbrook, G.C.S.I.; the Right Hon. Hugh C. E. Childers, M.P.; the Right Hon. G. J. Göschen, M.P., Vice-Admiral the Right Hon. Lord John Hay, K C.B.; Admiral the Right Hon. Sir John Dalrymple Hay, Bart., M.P., C.B., D.C.L.; the Right Hon. W. H. Smith, M.P.; Admiral the Hon. Arthur Duncombe; Sir W. G. Armstrong, Kt., C.B., D.C.L., F.R.S.; Sir Thomas Brassey, K.C.B., M.P.; Admiral Sir Alexander Milne, Bart., G.C.B.; Admiral Sir Frederick W. E. Nicolson, Bart., C.B.; Admiral Sir Cooper Key, G.C.B., F.R.S.; Sir E. J. Reed, K.C.B., F.R.S., M.P.; Admiral Sir Robert Spencer Robinson, K.C.B., F.R.S.; Admiral Sir W. Houston Stewart, K.C.B.; Rear-Admiral Thomas Brandreth; Mr. Nathaniel Barnaby, C.B.; Mr. Thomas Chapman, F.R.S., F.S.A.; Mr. Peter Denny; Mr. C. W. Merrifield, F.R.S.; Mr. George W. Rendel; Mr. J. D'Aguilar Samuda; Mr. W. H. Tindall; Mr. James Wright, C.B.; Mr. Joseph Woolley, M.A., LL.D., as Vice-Presidents; Mr. J. D'Aguilar Samuda, Treasurer; and Mr. John Corry and Mr. James Mackenzie as Auditors.

The PRESIDENT then nominated Mr. G. B. Rennie and Mr. P. Watts as Scrutineers to examine the voting papers.

The PRESIDENT: Gentlemen, in rising to open our Twenty-fourth Session I must ask your permission before I say anything more to give a notice. An important and interesting event takes place, as you are all aware, to-morrow—I believe something after five o'clock in the afternoon—in the shape of the Oxford and Cambridge boat-race, and it is proposed in view of that, for the convenience of our members, that we should meet at eleven o'clock if you do not object. Of course an hour gained in the morning is a very great gain. I have also to give notice that Mr. Butler's paper will be taken first, and Mr. Thornycroft, who has a special interest in the Thames, will read his paper to-day. I think it is necessary to give that notice, and I hope it may meet the convenience of many members.

Now, gentlemen, I must ask for your kind attention for a short time while I take a brief retrospect of the past year with regard to matters of interest to this Institution, and I hope entirely within the scope of our objects and purposes. Of course the first event naturally that is of the greatest interest, both to naval architects and likewise to the artillerist—and those two great studies

* For List of Members of Council actually elected, see p. xxix.

are so closely and naturally connected that it is impossible but that they must run side by side—is that of those warlike, or rather those naval operations which occurred at the end of last summer in Egypt. I purposely call them naval operations because I believe it is not considered quite accurate to describe them as warlike; but whether they be naval or warlike, they at any rate are of the deepest interest to all of us and to the country on three main grounds. First of all, they are the first naval operations which this country has conducted for thirty years; secondly, they are the only instance upon record where ships and forts have been in conflict armed with modern artillery; and thirdly (certainly not the least reason), they were quite sufficient to test very fully the entire competence of our highly instructed naval officers and their disciplined and gallant crews to navigate and to manœuvre the most powerful and the largest of our warships in narrow waters, and under circumstances of difficult navigation. And I think it must be a source of the utmost satisfaction to those eminent men who compose our constructive departments to think that no serious casualty, no casualty of any moment whatever, has been recorded in the whole course of those operations.

Gentlemen, it is not for me, of course, to offer, or venture to offer, any predictions as to the future of naval warfare, but I may be permitted—it is open to us all—to note the opinions of others on that point. To me one thing appears to be pretty clear: that in the long conflict that has gone on between the gun and the armour the gun has got the best of it; that no vessel can carry armour which is capable of resisting the gun that can be carried. And I also observe a very growing consensus of opinion, not only in this country, but in France, in Germany, and even in Italy itself, I think, in favour of what we call in these days a moderate type of warship—(9,000 or 10,000 tons we think is a moderate ship now)—rather than in favour of rushing into the extreme types, such as the *Inflexible*, not to speak of such gigantic vessels as the *Italia* and the *Lepanto*. I think, also, that there are not wanting some who incline to the opinion that rapid concentrated fire from guns of great penetrating power is to be preferred to the fire, or may be more efficient even than the fire, of a few guns of the very largest calibre, because I think experience seems to show that smoke is likely to be a very serious obstacle to accurate fire at great distances, and especially when big guns are enclosed in turrets. The great object apparently sought to be obtained now is concentration of fire, the power of pouring a vast mass of metal upon a given space either from guns placed *en barbette* or in battery; and if accuracy can be attained at sea, I confess it seems to me very difficult to understand how any ship can escape that is exposed not only to the surface fire from high altitudes, in some cases plunging fire—concentrated fire—from broadside or battery, but also to submarine fire in addition; it seems a very difficult thing to know how a vessel can escape. That is one of the most important points, I think, in the whole of the questions which surround naval warfare, viz., accuracy of fire at sea, and upon that great subject the late war has furnished us with no experience whatever.

Now, gentlemen, I pass from the Royal Navy to the Merchant Navy, and I am most happy and thankful once more to be able to congratulate you all upon the prosperity and the development of that great industry during the past year. I would remind you, and it is important to bear in mind, that the comparison we have to make is made with two previous years of unprecedented prosperity, each of them, as compared with previous years, and that makes the excess which this last year shows even more surprising than perhaps it otherwise might be.

I should like to say first of all that if I allude to certain ports only, I wish particularly that you should understand that it is not to draw invidious comparisons between one port and another; that is the furthest from my thoughts; but it is to save time; and in referring to two or three of the chief centres of the shipbuilding industry it is simply to give our members, and through our pro-

ceedings the country at large, some idea of the magnitude of the mercantile navy of this country and of the importance of that interest, and it gives us a very good idea of what it is doing and has done in the past. That is my sole reason for referring only to a few ports; it is only for the purposes of my argument.

The Clyde comes first, with 297 vessels of 395,140 tons: that is the largest total that has ever been recorded, and very nearly double that of 1879. Then comes the Wear, and I am proud to think that a north country river holds as a shipbuilding port the second rank in the world at this moment; the Wear built 123 vessels of 212,464 tons. Then comes the Tyne, with 132 vessels of 208,406 tons. Fourth, come the Hartlepools, with 39 vessels and 68,067 tons; and, fifth, the Tees with 40 vessels and 65,048 tons. I have here returns which I think are very interesting and extremely important as showing the productive power of this country—returns of the output of individual firms; but I daresay, for the sake of time, you will allow me to take them as read. They are very instructive and very important, but it will save time if you allow me to put them in afterwards. Now, taking the four great north-eastern ports of the Tyne, the Wear, the Tees, and the Hartlepools as one group, they appear to have turned out a gross tonnage of 333 vessels of 553,985 tons against the Clyde's 297 vessels of 395,149 tons, that is to say, 36 vessels and 158,836 tons in excess of that magnificent port of the Clyde. But I should like in reference to the Clyde to observe this, that the use of steel is very largely on the increase in the Clyde, much more so than in any other port with which I am acquainted. On the Clyde in 1879 the steel tonnage built was 18,000 tons only; in 1880 it was 42,000 tons; in 1881, 66,600 tons (this is steel tonnage only); and in 1882 the steel vessels were 60 in number, and the aggregate tonnage was 108,254 tons. The port of Barrow turned out 42,265 tons last year.

Now, I wish to refer to Belfast. Belfast is a very rising port, and it built last year 28,120 tons of shipping, as against 13,893 tons in 1881, and in the beginning of this very year Belfast launched the *Ionic*, a magnificent steel vessel of 4,700 tons, and I believe the largest sailing ship in the world, the *Lord Downshire*, was built at that same enterprising port last year. I say this because there has been a great deal said as to the absence of industry in Ireland, and the want of capital and so on; but it is an immense satisfaction to me, and it will be to all of you, to know that the greatest national industry (for, of course, I claim that title for the shipbuilding industry of this country) has taken a firm footing, and is showing a magnificent development in the port of Belfast.

Gentlemen, I am sure you will forgive me for referring to that, because it is a very satisfactory feature in Irish history. These returns are furnished by the builders themselves, and they are actually the tonnage launched during the year; but in order to arrive accurately at the increase of our carrying power, which is the great subject for the nation to consider, you must make some important deductions. You must deduct all vessels that have been removed off the Register, either broken up or lost (and I am afraid that represents a very large portion), and also vessels sold to foreigners. You must make that deduction on the one side of the account; but, on the other, you must take this most important fact into consideration, that a very large and immense proportion of this steam tonnage has replaced old sailing tonnage, and the amount of work that one ton of steam tonnage does as compared with that of a sailing ship is as one to three; some put it higher, even so high as one to four; and, therefore, when you take that into account, you will be able to estimate with some degree of accuracy the gigantic development of our carrying powers in this country.

Gentlemen, you will admit with me, I think, that these are most interesting and satisfactory returns. I compared the other day the returns that I had, and find that the total tonnage built was about 1,000,000 tons last year. If you deduct from that the vessels removed from the Register and sold, the actual increase to our carrying power I make out to amount to 554,000 tons

on the year, and on looking at those valuable statistics published by the surveyors of Lloyd's I find that my figures and theirs are almost alike; therefore I have very little doubt that the figures I quoted to you are fairly accurate.

That they are satisfactory I am sure you will all admit; but they give rise, gentlemen, to some reflections which I am not at all sure are quite so agreeable as the subject which calls them forth. The first reflection is, the growing increase, the alarming increase, of loss by collision and accident by collision. Hardly a week passes, sometimes much less, but we hear of some terrific loss by collisions, and it almost appears that the very efforts that we are making with such success to improve the type of ship, to gain speed, power, and strength, add to this danger, because a touch from one of the great merchant steamers of the present day is quite enough to send another to the bottom with all on board. Now, a great many suggestions are made from time to time as to the best remedy for collisions—to avoid collisions, or rather to reduce the danger to a minimum. One is a very obvious one, that all ships should be built in compartments. Well, but, gentlemen, unless the compartments are absolutely water-tight, I apprehend there is very little use in dividing the ship's hull into compartments, and I am very much afraid in many cases they are not water-tight. Another remark I would like to make is this: it is very little use to build bulkheads, unless you carry the bulkheads high enough in the vessel, at least up to the main deck, and I am afraid that is not always done. Then there is another suggestion that struck me the other day as a most important one, and that is, that the electric light should be adopted on board our large steamers at any rate, because I apprehend that in small cargo steamers the thing is impossible, on account of the expense and the absence probably of the power of creating the light. But I was talking on this very interesting subject the other day to a great friend of mine, who probably knows as much about the management of the large steamers as any man living, and I put to him this question of the adoption of the electric light. He said, "I adopted the electric light on board one of my own ships some time ago, and the first thing that happened was (I think it was in the Channel, but I am not quite sure about that) that another ship took my ship for a lighthouse." Well, that was rather awkward. Then there is another suggestion that has been made, and I must say it appears to me to deserve the attention of scientific men very much, that a ship's lights should be so arranged as to form angles by which not only the position of the vessel at the time but the course she is taking should be known. Well, I put that to my friend, and he said, "Yes, it is very well, but remember this, that if you begin to change ships' lights, all ships must change at the same time; not only English ships but foreign ships, or else if there is confusion now you will make it worse confounded, and possibly incur dangers that you know not of." Well, I must say I thought there was great sense in that; and the same remark, I apprehend, applies to the rules of the road, because even the rules of the road occasionally appear to me to cause the very collision that it is expected they will prevent; so that changes in matters of this kind of course have their dark side.

Now, gentlemen, the next reflection is one upon a weightier matter even than that to which I have just alluded, because we must remember that exactly in proportion to the magnitude of our carrying trade in time of peace, will be the responsibility of protecting that trade in time of war. Here is our vulnerable place; here is where the attack will be made, and the mercantile marine, you may depend upon it, will be the very reverse in time of war of what it is in time of peace. It is our strength in time of peace; in time of war it will become our weakness. But at the same time I am perfectly willing to admit that there is a large reserve of defence within the mercantile marine itself—there is no question about that; but, gentlemen, if the line of battle is weak, that defensive reserve will do nothing whatever to strengthen it, because the ships are not of the character to meet armoured vessels; and therefore I do not altogether think it a comfortable thing to be told, as we

are told—and I believe there is no doubt about it—that the growth of armoured fleets in some parts of the world is even greater than our own. I believe I am not far wrong in saying that the French have upon the stocks at this moment considerably more than double the number of armour-clad vessels that we have. Well, I do not want to arouse foreign susceptibilities in this matter, but I do want the nation to remember what the merchant navy of this country is doing. It is feeding the people, and it is employing the people ; and if we ever lose, for however short a moment of time, the command of the sea, no man living can predict the consequences that it will entail upon this country. In making these remarks I do not wish it to be considered as taking an alarmist's view : I merely want to look a great question (for it is a momentous question) in the face, and I wish the people of this country to take it to heart ; and, taking that view of it, I am inclined to agree, to a very large extent, with an old friend of mine, one of our most distinguished members, Sir Edward Reed. The other day, in a most valuable letter, which was not written in the least too forcible language, he made a protest against deducting and subtracting annually from the navy votes what I would like to term (and it is an expressive term which you will all understand) the dead weight of the non-effective services in those votes. Now, the estimates that were presented a day or two ago, and which will be discussed to-morrow, show most clearly the great force of Sir Edward Reed's protest. What do we find ? The total sum required for the naval estimates of 1883-4 is £10,757,000. It is quite true that that shows a nett annual increase over last year's vote for the navy of £129,567. But the building vote—that is to say, the amount of fighting tonnage that it is proposed to build this year—actually shows a diminution. It is very slight, but still it is a diminution of something about 500 tons. But when you turn to the non-effective services which I have described as the dead-weight, and against which Sir Edward Reed protests—not that they should not be paid, but that they should be deducted from the sums required for the maintenance and development of the fleet, that is the point—they amount to no less a sum than £2,071,400, and if you add to that the vote for the conveyance of troops, which is not a naval vote at all, £136,300, you come to no less a sum than £2,207,700, which must be deducted from the vote, which I say is the vote for the development and strengthening of the navy, and that leaves only £8,550,000 to be expended upon the development of the Royal Navy.

Now, gentlemen, one word only about the Admiralty programme. I am not speaking of one Admiralty or another, but my great objection to the Admiralty programme always has been this : that, in the first place, it is hardly ever completed ; and, secondly, it tells you what you will have five or six years hence, not what you have got now ; and in the present state of shipbuilding enterprise, whether war ships or not, I want somebody to tell me how many ships, at the end of five or six years—because the *Inflexible* took more than six years to build—will you have to knock off the other end of the list as having become obsolete ? That is the only way to get at what we may be some few years hence, and that very serious question, I think, has always to be considered in trying to ascertain what our naval strength really is.

Gentlemen, I really think I ought to ask your very kind forgiveness for the length of time I am detaining you ; but there are just one or two points—they shall be very short—to which I feel bound to allude. The last year unquestionably will find a place in English history as the year of naval exhibitions. There have been no less than four, but I will only allude to two of them, because they are of special interest to ourselves. I mean, of course, first of all, that great exhibition (our Report alludes to it) that was held in Fishmongers' Hall, and I congratulate the Shipwrights' Company upon the brilliant success of that exhibition ; and I am sure that nobody who attended it can fail to have been, as I certainly was myself, struck with the extraordinary beauty and finish of the foreign models that were exhibited there. I also think that our boat-builders almost might have learnt a

lesson that was set something like one thousand years ago in the building of the Viking boat, because a more beautiful open boat for sea-going purposes it is very difficult to conceive; it was a most interesting thing to see how very clever they must have been a thousand years ago for the purposes that they then had in view. There was one thing that was pointed out to me by a very eminent Admiral—I only just throw this out as a suggestion; I am not going into it fully, but I do not know whether any of our members were struck with this—that some of the foreign models were fitted with convex armour in the shape of a belt. Of course, convex armour in the shape of decks is used, but I do not ever remember having heard of convex armour being put in the shape of a belt upon any of our vessels. It was a distinguished Admiral who pointed it out to me, and it was one of the most interesting things, I think, that was exhibited there. I cannot remember whether they were French or German ships, but there were some models fitted with convex armour belts.

Now I have alluded to the Exhibition in London, not in too high terms, I hope. The next exhibition I had a little part to play in—and I must say, as a North countryman, I felt very proud of it—and that was the North-east Coast Exhibition that was held at Tynemouth. During the five weeks that it was open very nearly 250,000 persons—nearly a quarter of a million persons—visited it, and on two occasions (on September 16th and 23rd) no less than 20,000 people attended that exhibition. Now that is a very interesting circumstance, because it shows, I think, the value of these exhibitions. The amount of public interest that they excite tends to create a strong sympathy in the mind of the nation itself, with the interests and the progress of the mercantile marine of this country, and that is no unimportant thing to do; because my belief is that, if any Admiralty chose to ask the country to vote a little more money than it has been in the habit of doing for the purposes of the Navy, our wise people would not object to any such expenditure. That is my belief. It is all very well for captious economists to shout about economy, but they really do not understand the meaning of the word; because if any severe blow was inflicted either upon the Royal Navy or upon our Merchant Navy, the great interests which they are there to represent would be the very first to suffer, and they might not recover it.

Gentlemen, only one more remark have I to make. I had great hopes till the other day that the vexed question (in which I from circumstances have been a good deal mixed up, in former days particularly) of the load-line, was likely to be settled; and likely to be settled, moreover, by a body of persons who, I think myself, are the most competent by far to settle it, if it is ever to be settled—I mean the Committee of Lloyd's, assisted by the leading shipowners and shipbuilders of this country. I certainly had entertained those hopes, but they were a little dashed on Thursday last, because I found—in one of the most important speeches, by the way, that I recollect for a very long time, and coming from a high official person, as it affects in many ways the mercantile marine of this country—it is impossible to over-estimate its importance—I found that the Board of Trade demur to the acceptance of the Rules that Lloyd's Committee had prepared, at any rate at present, but I was gratified to see this: that the President said, "It is impossible at present to lay down an absolutely satisfactory and definite load-line which shall be in all cases compulsory." Now, gentlemen, in my humble opinion, no fixed load-line which shall be absolutely compulsory can by possibility be satisfactory at any time, because it cannot be just. A load-line must be fixed with reference to so many considerations; it must depend upon the structure of the vessel, upon the cargo, upon the time of year, and upon the service in which that vessel is engaged. When you remember, as we must all remember, that a vessel is a freight-earning machine, it is most important that the load-line be fixed, and fixed by competent persons—not Government departments, because I do not think that they are either competent to do it nor would they (certainly not the experienced Civil servants with whom I am acquainted) be willing to do it, because they know the responsibility that attaches to it

and the extreme difficulty of doing it. Therefore it is that I confess I rather regret that Lloyd's Rules are not likely to be accepted by the Board of Trade.

Now, gentlemen, I really owe you more thanks than I can possibly express for your kindness and indulgence in allowing me to make these few observations, but it has been an important year. There have occurred several matters which, I think, are quite within our purview, and I have ventured to touch lightly and most inadequately upon a few of them. I have only to thank you once more for your indulgence, to declare our Session open, and to call upon a distinguished naval officer, —who, I am afraid, you have all been waiting and anxious to hear—to read the first paper.

After the reading and discussion of Mr. Dunn's paper, the Scrutineers appointed to examine the Ballot Papers for the Election of the New Council presented the following Report :—

RESULT OF SCRUTINY OF VOTING LISTS FOR MEMBERS AND ASSOCIATE MEMBERS OF COUNCIL.

Society of Arts, John-street, Adelphi, W.C.
March 14, 1883.

To the President of the Institution of Naval Architects.

MY LORD,

We have the honour to report that we have examined the Voting Papers for the Election of Members and Associate Members of Council, and we find the following gentlemen have been duly elected :—

MEMBERS.

Barnes, F. K.	Kirk, A. C.	Swan, H. F.
Denny, W.	Laird, H. H.	Thornycroft, J. I.
Dixon, R.	Martell, B.	Warren, E. C.
Elgar, F.	Morgan, H.	Waymouth, B.
Gray, J. MacF.	Parker, W.	West, H. H.
Green, H.	Pearce, W.	White, J.
Harland, E. J.	Penn, J.	White, W. H.
Inglis, J.	Price, J.	
John, W.	Rennie, G. B.	

We also have to inform you that Messrs. T. Field and Andrew Leslie having received an *equal number* of votes, we are unable to add either of their names to the above list to make up the required number of Members of Council.

As regards the Associate Members of Council, we find, after removing the names of C. W. Merrifield and W. H. Tindall, who have been elected Vice-Presidents, that the following gentlemen have been duly elected :—

ASSOCIATES.

Ailsa, Marquis of.	Froude, R. E.	Rundell, W. W.
Bramwell, Sir F. J.	Kemp, Dixon.	Siemens, Dr. C. W.
Cotterill, J. H.	Ramsden, Sir James.	

We have the honour to be, my Lord,
Your obedient Servants,

(Signed) { GEORGE B. RENNIE, P. WATTS, } *Scrutineers.*

The PRESIDENT having remarked that it was only possible to decide between Messrs. Field and Leslie by lot, thereupon cast lots, when it was found that Mr. Leslie was elected.

Since the issue of Vol. XXIII. the following gentlemen have been added to the list of Members and Associates respectively: Messrs. James W. Claridge, Alfred A. Dietrich, John G. Dobbie, John Francis Hall, Andrew King Hamilton, John Lennox Lumsden, Edwin Lewis Martin, Henrick van Meerten, Hector MacColl, John Henry Morrison, Joseph Newnham, Robert James Quelch, George Henry Stainer, Alexander Taylor, Joseph Lowes Thompson, jun., Thomas Todd, Robert Zimmermann, Members; and John Clark, Albert C. A. Holzapfel, John D. Milburn, George Renwick, Frederick Stumore, and Henry J. Watson, Associates.

During the same period, the Institution has sustained the loss of the following Members and Associates: Robert Armstrong and B. J. Tideman, Members, and John Moffat and W. Symington, Associates.

ON CERTAIN POINTS OF IMPORTANCE IN THE CONSTRUCTION OF SHIPS OF WAR.

By Captain G. H. Noel, R.N., Associate.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 14th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

I should not have intruded upon your valuable time had I not felt that in this Institution —which comprises among its members and associates, not only the most eminent experts in the great engineering science of shipbuilding, but also the merest amateurs in the art— the opinion of naval men may be useful, and even valuable, since their duty is to command, and if necessary to fight, those vessels which the naval architect designs and constructs. Being in the position of an amateur, who has carefully watched and endeavoured to think out many of the questions relative to the construction of ships of war from a practical point of view, I have undertaken to read this paper, and submit it for discussion at this meeting.

The number of interesting and important considerations embodied in the science of shipbuilding is so great and varied, that it would be far beyond my power—even if time allowed—to touch on more than a very few of them. I therefore propose to confine myself to certain points, the great importance of which may possibly in some cases be under-estimated. They are:—

I. On the strength and height of the bow necessary for ramming.

II. On water-tight compartments.

III. On armoured conning towers.

IV. On torpedo defence.

I.—*On the Strength and Height of the Bow necessary for Ramming.*

In taking his ship into action, it is most desirable that a captain in Her Majesty's service be fully confident that the ram with which she is provided is of such construction that he need not fear the consequences of charging the enemy, should he have a fair opportunity of doing so. Without that confidence he would fight but a half-hearted battle;

and, considering the enormous weight of responsibility that rests on him, it is most essential that he should be able to fully rely upon the power and efficiency of his vessel, and of the weapons on which the issue of the fight must so greatly depend.

The late Mr. Scott Russell—a much-lamented friend and supporter of this Institution, whose memory is pleasantly associated with it by the thankworthy consideration of the Council in giving us an admirable portrait of him in the last number of our Transactions—in a paper read at the Royal United Service Institution, June 8th, 1877, speaking on this subject, says: "What we want in collision construction is that the strong thick parts of the ship should grow evenly out of the weaker thinner parts abaft, so that the strain of collision shall be spread over a wide space, thus utilising the strength of a large extent of good iron. For this purpose the stem must be formed by gradually growing out of the ship's bow, in plates overlapping thicker and thicker, until they become a thick, smooth, inflexible stem. The same principle must prevail throughout the whole bow of the ship. The decks must all be incorporated in the interior of the hull with the skin and the stem, so as to form an entire hollow weapon of great strength and homogeneous structure." These words should still have their full significance in guiding present and future construction; not that there is any want of care on this point, but because in the ever-increasing number of subjects to be considered, all involving new difficulties and complications, the obstacles in the way of providing the necessary strength for the ram increase, and the extreme importance of doing so is apt to be undervalued.

The nature of the strains brought on a bow when ramming is patent to all here. The dangers which would arise from imperfect construction or design may be classed as follows:—

1. Of the ram bow being actually forced in.

2. Of the bow being twisted and the stem broken.

3. Of the vessel being herself sunk, which might result from two causes—either the want of height in her bow and freeboard, causing her to lurch over and so capsize, or to go down head-first; or else, owing to weakness in the bow upper works, which, breaking away, might lock with those of the enemy's ship, and so cause both vessels to sink together.

With reference to the first danger: there is no difficulty to be apprehended in obtaining the desired strength in the bows of comparatively small vessels, so long as wrought-iron or steel stems can be used; but with increase of size and weight we get an increase in the momentum, and in some cases a reduction in the cushioning effect, which so much lessens the enormous strains consequent on ramming. For these reasons great additional strength is required in heavy vessels. It is questionable whether this is fully appreciated in the construction of citadel ships, where the stem—though to a small extent

backed up by an armoured deck—is unsupported by side armour, and the principal weights are concentrated in the centre of the vessel some distance from the bow, upon which the whole effort of bringing this mass to rest devolves when the ramming is direct. In some of the armoured and other cruizers the system of coppering the bottom over wood-sheathing has been adopted, necessitating the introduction of gun-metal stems. The prudence of this measure may also be questioned, as it is doubtful whether the necessary strength to withstand the shock of ramming can possibly be obtained with brass, even by using an additional weight of metal with the intention of supplying this deficiency. It would certainly be advantageous if some means could be discovered whereby a less brittle substance than gun-metal could be employed for the stems of these vessels.

Secondly, as regards the twisting of the bow and breaking of the stem. Examples of this danger are continually brought to our notice by collisions at sea, notably on the occasion of that disastrous accident which happened to two German ironclads in the Channel a few years ago. The constructors of English ships of war have wisely curtailed the length of the ram bows of our ships; a measure calculated to give them greater power to resist oblique or twisting strains; but is it not desirable in our heavier ironclads that a more secure *root* be constructed to the ram? This might be accomplished by building its point on to a cigar-end-shaped structure worked smoothly into the bow, and giving the sharpness requisite for the speed of the ship by its horizontal as well as its vertical entrance; in fact, like the lower part of the stem of the *Polyphemus*, without its torpedo discharge pipe.

Thirdly, as to the height of the bow required for efficiency as a ram, and the strength of the bow upper works. The first part of this question applies to coast service ironclads with low freeboard, denominated "rams," and which are often spoken of as being more serviceable in that capacity than larger and more unwieldy ships. Some of these "rams" are constructed with fairly high bows, and when fighting in smooth water would be most formidable; but no vessel of this type can give her captain the confidence he would possess were he commanding a ship with a high, well-constructed bow, which would ensure the enemy he had rammed being thrown off clear, and at the same time would cause him no misgivings as to the result of entanglement with his foe, or of a heavy and severe lurch after drawing clear of her. The strength of the upper works of the bows of high freeboard ships is undoubtedly sufficient in all cases except those of vessels constructed of light steel, where the danger may imperceptibly creep in of making the upper works forward too light to be able to resist the pressure brought on them by ramming—a weakness which, if not provided against, may lead to disaster.

Ramming is a science which cannot be practised; we are therefore dependent on accidental collisions—unhappily only too numerous—and a few cases of ramming in action, for information as to its effect. With a fleet such as that possessed by England, the tactics of ramming must ever be considered of very great importance. In using these

tactics every endeavour should be made to inflict the most fatal blow on your enemy with the least damage to yourself—that is, to ram scientifically. But since it is impossible to practically study the art, there is no doubt that in a ramming combat the shocks and strains experienced will be much more severe than would be the case were such practice possible. It is only natural, therefore, that the strength and efficiency of the bow for ramming should be deemed by naval officers a matter for most urgent consideration. This is one of the points I wish particularly to impress on the meeting.

II.—*On Water-tight Compartments.*

Water-tight compartments in a ship of war are required to prevent her from sinking after being severely wounded in battle, as well as, in common with other ships, when harmed by collision or grounding. The principal object is, therefore, to design effective water-tight compartments, the bulkheads or vertical sides of which can be closed in action without impairing the necessary communications, and without detracting from the draft and ventilation requisite for steaming at full power.

Were it possible to abolish all such dangerous appliances as water-tight doors the safety of ships would be vastly increased. This is of course impossible in central citadel ships constructed with armoured decks before and abaft the citadel a few feet below water, and may be considered to detract in some degree from the value of this type of ship. In belted ironclads there is no reason why the only communication with the principal compartments should not be from above, for where there is a convenient deck fore and aft above the water-line, the communications with and stowage of these compartments must be a simple matter enough. All pumping and ventilating pipes, drains and other breaks through a bulkhead are sources of weakness, and however well fitted with valves and cocks (whether automatic or worked by hand), are liable to be found faulty when the crucial test is applied. Compartments with intact bulkheads, communicated with, ventilated and pumped out only from above, and having their deck or decks fitted with water-tight hatches, would ensure the greatest possible degree of safety to the ships.

In armoured vessels of the central citadel type, where it is imperative that the armoured deck be intact, the only plan which seems to carry with it any degree of security is that of constructing before and abaft the citadel, "passages" or "shafts" in the centre line of the ship immediately under the armoured deck, having one end opening upwards into the inside of the citadel above the water-line; these passages extending as far as the last compartment at each extremity of the vessel, and being the only means of communication with the lower compartments aft and forward. The entrance to each separate compartment would have its water-tight door, and the pumping-out and ventilating pipes of each compartment would also be led directly into the passage. A shaft so placed would be in as secure a position as possible from the disruptive effects of a successful ram or torpedo attack. There might be difficulty in the stowing of the compartments,

but this plan would at least admit of all the bulkheads being intact, except where there are openings and pipes into the shaft, the doors and valves of which could be worked with a fair amount of security from the passage itself. Any compartment which could open directly into the citadel would have the same element of safety as those in belted ships, as before described.

One of the matters to be attended to in action is the safety of those men whose duties employ them in the lower compartments of the ship where torpedoes are worked, cartridges and projectiles are issued, and other business is necessary. An escape must be provided for them in case of their being flooded out, and such escape ought to be so arranged as to cause no great additional danger to the ship should a door or hatch be left open or unfastened in the hurry of their retreat. This escape would be perfectly provided for in belted ships where the compartments open only upwards, and would be fairly provided for in citadel ships fitted with central fore and aft shafts, as suggested.

Another source of danger which may appear in action, and which requires attention, is that of a panic in the stoke-hold. This may be said to apply principally, if not solely, to ships fitted for forced draught, where the stoke-hold is closed in air-tight, and the pressure in it raised above atmospheric pressure by means of fans. It should be considered essential in all ships that a means of escape be provided for the stokers up the funnel-casing, or some convenient passage; if this is not done, the knowledge that they are hopelessly shut in may lead to a panic, and cause great confusion, if not actual disaster. One of the chief anxieties to a commanding officer in battle will be to keep the necessary command of speed on his ship, as the failure of the engines at a critical moment might be fatal. This failure could result from several causes, not the least serious of which would be a panic in the stoke-hold.

The labour and ingenuity which have been devoted to the construction of water-tight compartments in ships of war, and the admirable results obtained, are most fully recognised in the service. Our recent ironclads—built with double bottoms, wing passages, and numerous internal compartments, all with well-strengthened bulkheads—are as safe as floating vessels can be: we may reckon on their withstanding the explosion of two, or perhaps, three Whitehead torpedoes under them, or even the shock of a ram, without being sunk. But there is this condition, *all door and valve connections between the compartments must be securely closed.* Even with this high degree of efficiency there still exist certain points, which, viewed from the position of a commanding officer about to take his ship into action or danger, may be considered wanting in perfection, and should, if possible, be rectified. Some of these points are the outcome of new designs; others are admitted weaknesses, which cannot be conveniently avoided; and a few result from over-confidence in mechanical contrivances, the practical working of which, naval men are inclined to view with suspicion. In the Navy we are trained from boyhood to act upon two great maxims, namely, "To take every precaution against accident," and "To arrange everything that

may have to be done on an emergency so simply, that there can be no likelihood of mistake or confusion." Now that vessels of war are yearly becoming more complicated in their build, we are naturally anxious to impress naval architects with the importance of carrying out like principles in the construction of our ships, with the view of rendering them as easily secured from harm as possible, when dangers really occur.

III.—*On Armoured Conning Towers.*

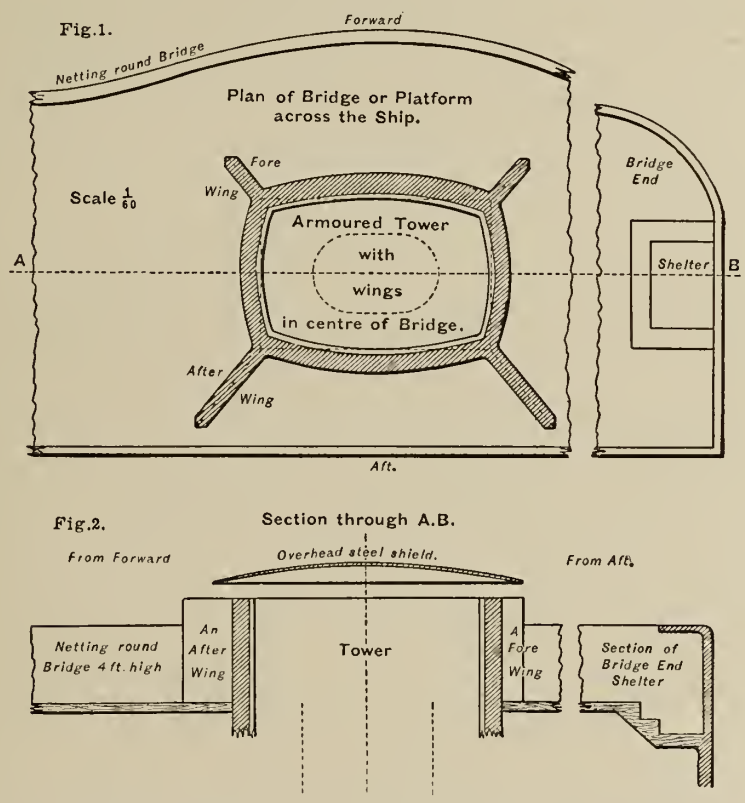
The conning tower, or captain's turret, is the place from which the ship herself is managed, and from which the various weapons are directed. A ship without an efficient conning tower may be compared to a man without an efficient head-piece; and yet, until quite recently, few matters affecting the fighting efficiency of the ship have had less attention paid to them.

Exposure to view, and, in some instances, the position of the weight to be carried, curtail the height of the conning tower; but, apart from this, nothing should be allowed to prevent its being placed in the position best adapted for an all-round view, especially clear ahead, and at the same time favourable for conning and steering the ship, for directing the guns and torpedoes, and for communicating with the engine-room, batteries, &c.

The following is a brief description of what may be considered an effective arrangement for conning in an armoured vessel (see drawing). A bridge or platform across the ship, strongly constructed, and having a bullet-proof parapet or netting around it, about four feet high, which will also protect the occupants from the concussion of their own guns. In the centre of the ship, and standing to a height of $5\frac{1}{2}$ ft. through the middle of this bridge, an armoured oblong tower, reaching down into the ship below the level of her side armour, and having above the bridge four armoured wings, which would provide shelter for the officers outside of the tower. It is seldom, if ever, possible to effectively con a ship when manœuvring with others from an enclosed tower, and it will be found even more impracticable to do so in a fleet action; the commanding officer will often find it necessary to station himself, and perhaps some of his assistants, on the bridge, and the protection of armoured wings, such as those referred to, may well be afforded, under which these officers may take shelter when passing close to the enemy, if only to shield them from the effects of the concussion of the opponent's heavy ordnance, and the searching fire of machine guns. The tower itself would be a structure of immense strength, and of sufficient size above the bridge to admit of room for freely working all the appliances necessary for steering the ship, directing the guns, and communicating with the different parts below; all such appliances being arranged with the utmost care and precision. The armoured wings on the bridge outside the tower would stand out from it at angles of 45° with the fore and aft line, those on the fore corners being about 3 ft. in length, and those on the after corners 5 ft.; this arrangement would least interfere with the "directors" for the guns. A bullet-proof roof or shield would cover the whole tower and extend over the wings,

the edges capable of being raised on either side, in case of the ship getting a permanent list. For going into action, the bridge must be entirely clear (any temporary chart-house or other obstruction being removed), it would extend sufficiently far towards the ship's side to command from its ends an unbroken view ahead and astern, and from it the commanding officer should have full control of all that is going on inside the tower. Duplicates of the tell-tales, indicating the movements of the engines and rudder, and the speed of the ship and engines (when these last are supplied) would be placed on the fore edge of the

ARMOURED CONNING TOWER.



NOTE.—Width of bridge or platform will depend on the nature of the vessel; and the depth to which the armoured tower will have to be taken, on the position of the side armour.

bridge, in conspicuous positions, and, lastly, bullet-proof shelters would be fitted at the bridge ends—which, however, must not show above the bridge netting so as to obstruct the view from the directors.

The essentials for efficient conning and directing arrangements may be thus summarised. A strong armoured tower with armoured wings at its corners, as complete as possible in all its internal communications and appliances; with a clear serviceable bridge or platform round it, from the position of which the captain has full control of the armoured tower, and is best placed for manœuvring and fighting his ship.

All ironclads are at present provided with conning towers of more or less efficiency, but there is a general want of completeness and uniformity in the arrangements, as a whole, for the serviceable conning and directing of these ships in action. Unarmoured ships are almost entirely without any system of protection for their conning arrangements and communications; it would be found extremely beneficial to such vessels were they fitted with towers and bridges similar to those above described, the towers constructed of a moderate thickness of steel, instead of being armoured.

As a matter of actual *construction* the conning tower with its bridge may be considered of comparatively minor importance; but in the *design* of a ship of war, its position should be early decided upon, and to it all other external arrangements, whether with reference to guns, funnels, boats or masts, should be subservient. I have said a good deal on this subject, because it so greatly affects the fighting efficiency of the ship from the captain's point of view.

IV.—*On Torpedo Defence.*

The necessity of supplying the ships of our fleet with means of protection against the attack of torpedoes is but too apparent, when we consider the gigantic strides that the methods of torpedo attack have recently taken; the immense increase in the number of torpedo craft, some of unparallelled speed; the wonderful results obtained with Whitehead torpedoes; and, the fact that every nation with a sea-board is arming itself with these destructive weapons. In maritime warfare, every state whose navy is not sufficiently powerful to keep the sea against its adversary, will cultivate the art of attacking with torpedoes; while those nations whose naval power admits of their sending fleets to sea, must study the resistance of such attacks, or in other words, "torpedo defence."

Torpedoes may be classed in general terms as stationary and locomotive. The former class is practically unlimited in destructive power; such torpedoes must, therefore, be rendered harmless before an ironclad or other important vessel should be allowed to approach within their range. The latter class is fortunately limited in power, and it is against these weapons that we have principally to provide protection. Apart from any strengthening measures which may be taken in the construction of the ship below water to enable her to resist the explosion of submarine mines, there are two methods of defence against locomotive torpedoes. One by rigging out obstructions and hanging nets round the ships (this, assisted by machine-gun fire, is the only defence a ship can provide from her own resources), and the other by means of counter-attack vessels of equal or nearly equal speed with torpedo boats, but capable of keeping the sea and cruising with the fleet.

In a secure anchorage it would be easy enough to arrange effective protection in the way of obstruction, supplemented, if necessary, with lines of torpedoes round the ship or anchorage; but it must be borne in mind, that a ship so protected sacrifices her mobility, and

may become the victim of a ram before she can get under weigh and be clear for manœuvring. In order to constitute a serviceable defence, nets and obstructions should be capable of being got in and out quickly; they should be strong enough to be carried at sea (when the nets on the beam of the ship might be rigged out if required, provided that the weather was moderate), and the gear should be so arranged as not to mask the guns at any time. Unless this system of defence is most carefully and serviceably fitted, the dangers of fouling the screw, impeding the ship's movements, and otherwise detracting from her fighting efficiency, are such, that many officers would sooner risk being without it. However, so great is the importance to England of having her valuable battle-ships protected from the attack of torpedoes, that it may be hoped no pains or expense will be spared in designing and supplying our principal vessels with effective torpedo net defence.

As regards small sea-going vessels of great speed to accompany the larger and more unwieldy ships of the fleet as auxiliaries, it is fast becoming recognised that the need of some such vessels will be urgently felt in time of war, and that it would be well if every battle-ship in a squadron had one or more of these small craft attached to her, as a tender to protect her from surprise and, perhaps, the successful attack of swift torpedo boats or vessels, which her own unwieldiness might prevent her from combating. Captain R. H. Harris, R.N., read an admirable paper on the subject, at the United Service Institution, last year.

Some small swift vessels, commonly known as the Rendel type, have lately been constructed at Sir William Armstrong and Company's works, and proved a great success. The class which is called at Elswick "Cruiser A" consists of vessels of only 1,360 tons displacement, carrying two 25-ton and other small guns, and realising a maximum speed of no less than 16·4 knots, besides which they are reported to be sea-kind and serviceable craft. To nations whose battle-ships are to keep the sea in time of war, vessels of a similar type—we may almost say—are *indispensable*. Such vessels should be limited to 1,200 tons displacement (or less for the comparatively smooth waters of the Mediterranean), they should carry an armament consisting chiefly of machine shell guns, and torpedoes, and possess the highest speed possible for a vessel intended for sea-going work. Their three essentials are: (1) Great speed; (2) Small size; (3) Sea-worthiness.

The protection against torpedoes which comes most directly under the heading of this paper, is that obtained by giving greater strength to the bottom of the ship, with a view to its resisting the destructive effects of submarine explosions. So long ago as 1869, Sir Edward Reed propounded and established the principle of constructing the hulls of armoured ships as lightly as possible, so that a greater proportion of the weight of displacement might be devoted to armour, armament, equipment, &c. Torpedoes were then in their infancy, and the idea was to submit to having the outer skin of the ship blown in by a torpedo, and to trust to double bottom and other compartments to prevent her being seriously damaged. This principle appears to be still in vogue; but since locomotive

torpedo attack has now so greatly developed, would it not be prudent to reconsider the question, and put more strength into the construction of the ship's bottom, even at the expense of reducing the weight of armour to be carried? We cannot hope to obtain sufficient strength to resist the effect of a charge of dynamite or gun-cotton ignited when actually in contact, but with stronger bottoms, and perhaps the introduction of some plan by which the blow on the outer skin might be cushioned, a fair amount of safety would be secured against such explosions, when the mine is not in contact. So much has recently been done to organise and perfect the attack on the vulnerable parts of the ship below water, that we may well think seriously of introducing some such modification in the construction of the hulls of our armoured ships. Whilst on the general question of defence, we, in common with other nations intending to hold the sea against their adversaries, must be prepared with serviceable, well-fitted nets and obstructions; and counter-attack vessels thoroughly suited for the work; to resist torpedo attack in every variety of form.

From the numerous subjects of interest bearing on the construction of ships of war I have selected these four, because they appear to me to be among the foremost in importance from a naval point of view. The strength of the ram, and the system of water-tight compartments, are now old questions, but age does not detract from their value, and they must not be allowed to rust, while the necessity of having efficient conning towers and torpedo protection becomes increasingly apparent in proportion to the improvements in the accuracy and power of heavy ordnance and machine-guns, and the development of the attack with torpedoes. I have endeavoured to point out where improvements might be convenient, and where weaknesses may be experienced when our ships are tested in action, but I shall be perfectly satisfied if I have succeeded in impressing the meeting with the importance of giving these questions full consideration. I trust some of the gentlemen present will freely, and, if they think fit, severely criticise this paper.

DISCUSSION.

Sir Edward Reed: My Lord and Gentlemen, I think no time should be lost, at any rate by the shipbuilding members of this Society, in recognising the unusual value of this paper. It appears to me that it is a paper of a very exceptional kind with us, but of precisely the kind which was contemplated from the very foundation of this Institution. It was hoped that one of the great objects which this Institution would effect would be the bringing within reach of each other the shipbuilder and the sailor; and I am sure that this paper has brought the sailor in our midst to-day with very considerable advantage on three or four points that must have exercised the minds of all designers of war vessels. I agree with the author in thinking that he has selected some of the most important points demanding consideration, in the choice of subjects which he has made. With regard to his observations on the strength of the bow for ramming, I am inclined to think that the subject requires a little more consideration, as is not unnatural, than he has given to it. In the first place, in the only

experience that we have had, at any rate that I have been able to study myself—the only practical experience with ramming—some very curious and unexpected facts were developed, and one which I would recommend to the careful attention of Captain Noel, because I notice that in the paper he suggests that citadel ships are peculiarly liable to weakness and injury at the bow when used for ramming, in consequence of the absence of armour-plates at the bow to back up the stem. Now that is a very natural opinion to form, but I am sorry to say that in the case of the ramming which occurred between the two German frigates, it was proved that the presence of armour backing up the stem was precisely the cause, taken in conjunction with the enormous protrusion of the stem below the water, of the great amount of injury which happened. What occurred was this: the ramming ship took the other ship obliquely, and both were at speed, and that being the case the under-water point of the ram, which, as I say, had great advancement, was seized as it were by the ship rammed, and the armour which came to back up the stem at the water line became a fulcrum, and consequently the momentum of the other ship acting at the point of the stem, tended to, and did, turn the whole stem about the armour ends. The armour at the water-line served as a fulcrum, and the stem was wrested out from all its connections, and resulted in a most frightful displacement. Now I only suggest this: it is not at all as an expression of the desirability of having no armour to back up the stem, but rather as an indication that unless the matter is very fully thought out, and thought out in relation to the form of the stem as well as to the fact of the presence of armour, and to the width of the armour which backs up the stem, you might possibly make great mistakes. Another remark touching the ram in which I cannot quite concur with Captain Noel, is the intimation which he gives that in his opinion the rams of the composite vessels sheathed with copper and built with brass stems and stern-posts are brittle and weak. I think that they are not by any means brittle, and I very much doubt whether it would be found in action that they are not amply strong enough for all their purposes: I mean, regarded as solid blocks of metal; I am not speaking now, of course, of their connection. There is a point to be carefully considered in that connection, and it is, I think, a very serious one, touching that type of ship; and that is, that you must expect—at least I expect—that in all cases of one ship ramming another at speed there will be great disturbance at the bow, however well you build it, and when you set up disturbance at the bow of a composite vessel sheathed with copper, you set up an opportunity for the copper bottom attacking the iron of the ship and extending the mischief by chemical action from the moment the ramming takes place. That is a serious point, which deserves more consideration, and I confess that for my part I adopted originally in the *Inconstant* the combination of a copper bottom with an iron-plated bottom with great apprehension, and I view with some objection and some fear its greater extension, and as far as I can I avoid the adoption of copper bottoms with an iron-bottomed ship; I do not like the combination. In saying that, I do not wish at all to say that those do wrong who adopt that combination, although my own feeling is against it. Coming now to the next point (and our Rules make it very difficult to do justice to a paper of this kind in so short a time), I would venture to say that Captain Noel has expressed on the question of water-tight compartments, and the various complications that now occur in connection with the working of a ship, an apprehension which I am not at all surprised at, and one which has given me great anxiety at many times. I very much doubt whether the modern ship of war has not become, by complication in every part, a machine which men cannot efficiently master and handle in action. Take the case of the one ironclad which we lost by collision, the *Vanguard*, as an illustration of the manner in which even simple and obvious duties are likely to be lost sight of in the hour of battle. How was that ship lost? She was lost from the neglect of the most obvious precaution after her collision occurred, namely, the closing of the water-tight doors between the engine and boiler room;

and, moreover, in that case the chief engineer of the ship was in charge of that duty, and it was the occasion when, if upon any occasion, you might have expected the thing to be well done; and yet the responsible officer tells us that when the accident happened he rushed out and saw water pouring into the engine room, and (instead of applying himself to the closing of the water-tight door, which was the one thing to save the ship) he ran on deck and told the captain that in his opinion the ship would go down. Well, with facts like that before us, how can it be expected that men will be found, whether officers or executive seamen, to perform all the things which it will be requisite to perform in the case of all the accidents of a naval war, and under all the excitements of action? I remember a famous Admiral of the American service, Admiral Farragut, speaking to me upon that point, and almost beseeching me—after we had made a visit together to Chatham Dockyard on one occasion when I had the honour of being at the Admiralty—to keep things as simple as possible, and warning me not to depend upon either officers or men handling complicated things under such circumstances. Why, my Lord, just imagine what must be the nervous condition of any man not gifted with extraordinary nerve in entering upon an action in command of an ironclad ship, with the knowledge that he has half a million or three-quarters of a million of money beneath his feet, together with a great many lives, and that in the only instances in which one big ironclad has rammed another at speed the other has gone to the bottom. I say that the anxieties of naval men in that position must be very great, and I think it most desirable that they should come here and express them to us frankly and fully, as Captain Noel has done in this case, and I think we should lay their suggestions to heart. Now I should like to say a word or two upon conning-towers. I would like first of all to say that this section of the paper is peculiarly gratifying to me, because it is now twenty years that I have been endeavouring to elicit from naval officers exactly what we have here got. Several times over when I was at the Admiralty, as I think the right hon. and gallant gentleman sitting there [Admiral Sir John D. Hay, Bart., M.P.] will bear me witness, it was a matter of anxiety on the part of the Admiralty to elicit from the admirals and captains in command of the ships of the Channel Squadron suggestions with regard to what ought to be the character of the conning-tower on an ironclad line-of-battle ship, and what we always got was a variety of suggestions, and what we seldom got was a confident opinion and a defined plan; in fact, we never got one, so far as I can remember. I think myself it is a question pre-eminently for the naval officer to tell us how he would like to be situated and circumstanced in action in commanding and fighting one of these ships. Now, I may say that the Admiralty have not been wanting, in my opinion, in this—if my gallant friends present will forgive my saying so—so much as the naval officers. I remember in the case of one ship I put a tower which was broad enough—it was placed abaft the funnel—to enable you to see past the funnel on ahead quite clearly over the bow; you saw everywhere else quite perfectly; it was high enough to look freely over the forecastle, and over the boats and everything else. It was large enough to contain a steering wheel, and the various signals, and in fact was what I would call the embryo idea of the more elaborate suggestion which Captain Noel has made; and the only result I got for my pains was that the then senior Sea Lord of the Navy went and rather derided this tower on board the ship, and said how much better it would have been if the weight of that tower had been put in small guns along the upper deck. That is an illustration of one of the difficulties we have had. I do not much complain of this, my Lord, because everyone must see that the problem is of the most difficult kind possible; and that it is one upon which naval officers, like ourselves, must of necessity have differed materially during the fluctuating conditions of the naval service in times past. I should like to say, further, that I very much approve of this tower of Captain Noel's. I think most highly of it, and I take a very lively interest in it. I remember having some discussion on this matter with a very eminent officer of a foreign service, the late Prince

Adelbert of Prussia, when we were engaged in building the *Kaiser* and the *Deutschland*, which Mr. Samuda constructed for that Government, and with which I was connected. We had a great deal of discussion in Berlin about the conning-towers of those ships, and his Royal Highness came to the conclusion—at least he was very strongly of the opinion which Captain Noel has expressed, namely, that although a tower is most valuable and indispensable for certain purposes, I mean for being within it—it is very necessary to have outside armoured shelters, so that officers can move from side to side, and so that they can even meet together in temporary security outside of the tower under many of the conditions of naval warfare. Without further dwelling upon it, I would say that as far as I can see, the tower which Captain Noel suggests is a most admirable one, and thoroughly well deserving the consideration of all of us who are engaged in works of this nature. I am afraid that I have taken up my ten minutes, but I should like to make one remark upon the subject last touched upon in the paper, that of torpedo defence. Captain Noel says that as long ago as 1869, and he would have been quite correct if he had said as long ago as 1863, I assisted, or at least took part, in establishing the principle of saving as much iron as possible in the merely floating bottom of the vessel to get it for the armour and other things; and a good deal of trouble we got into, because people who are often more ingenious than they are kind, discovered, in the fact that we had saved a little more than we had calculated upon in the one case, and required a little ballast to replace it, a reason for attacking a class of ships and their designers incessantly, and misrepresenting them in every possible way; and not only that, but for turning what I believe was a professional triumph into a means of professional discredit and disadvantage. But I mention this point for the purpose of saying that my thoughts run rather in a different direction and towards a different result —I do not know that they will continue to do so, but they do at present and have done for some time past—from that which Captain Noel suggests. The idea is, that we should make this outer bottom of the ship thicker. Now my idea is, that whatever we can do in the way of thickening the bottom of the ship, it should be the inner bottom that we make thicker, and that we might make the outer bottom thinner with advantage in certain cases, giving the iron so saved to the inner bottom; and my reason is this: that the inner bottom is further away from the torpedo than the outer one under whatever circumstances the torpedo is exploded, and the further you can get the torpedo to explode from your strong bottom the better. Therefore I incline to think that it is possible we may come to this: we may come to an internal bottom which would be much stronger than bottoms now are, and that we should make the external bottom—not a net, of course, that would not be possible—but such as to perform the function which the net performs at some distance from the ship—to perform that at some distance from the inner bottom of the ship. I do not put these views forward as at all final or conclusive; they are merely suggestions which have occurred to my own mind. Having exhausted my time, I will only repeat, my Lord, that I for one am extremely pleased with this paper, and very grateful to Captain Noel for having read it.

Mr. J. D'A. Samuda: My Lord, I entirely concur in all the observations which Sir Edward Reed has made with reference to the ram, and I would merely add in confirmation of what he has stated that the apprehension expressed by Captain Noel in this paper may be quite put on one side with reference to the material of gun-metal not being suitable for its application in the position of the ram when wood and copper have to be used in conjunction with a steel-built vessel. The difference in strength between properly constructed gun-metal—properly mixed gun-metal—and iron is very much less than Captain Noel could have imagined when he wrote this paper. I am testing some myself at this very moment on a ram of the particular kind which he has described, and he will possibly hardly believe it when I tell him that the breaking strain of that gun-metal has

been shown to be over 18 tons to the inch (that of ordinary iron being 22 tons), besides having a good deal more elasticity. Therefore, I think he may altogether disregard the idea that gun-metal will have any bad influence whenever it is desired to use it in the construction of vessels of this sort. I do not think that this would be perhaps quite the right time to go into one's views with regard to the application of the ram generally as an offensive power in the Navy; but for my own part I hold that ramming will be very little used, and very much less successful than guns when it is used; and you will find, as a rule, that what Captain Noel has expressed here is again quite right, that it is not a science that can be practised very much, and that, consequently, deductions cannot be drawn from it very easily. As to the extent to which it has been used, Sir Edward Reed has mentioned one case to which I do not care to allude again, but I should just like to mention another in confirmation of the view I have expressed with regard to it, and that I received from Admiral Tegetthoff, who, I believe, is the only man who has used the ram in actual warfare. He described to me in the most graphic way the effect produced upon him by having rammed the *Ré d'Italia*, which you will recollect was sent to the bottom by his vessel at the battle of Lissa. He said, "If I were to live a thousand years I never would ram another ship, the impression produced upon me was so awful." It was not only the effect produced upon him as being singular, because my own impression is that he was one of the coolest and bravest men I ever spoke to; but he said, "I went into that action having given orders to every captain under me, when I made a certain signal, to turn his vessel and ram the vessel opposite to him, and the only one that rammed was my own. After I had done it the effect produced was different from anything you have in naval warfare generally. You see the vessel attacked at one moment, and the next moment after she is struck 800 men sliding noiselessly into the sea, with the vessel following them, and then find yourself left with a perfect void, without any commotion, without any smoke, without anything to make one feel that one was in battle. All these things created such an impression upon me that under no circumstances would I ever ram again." Well, I cannot think, when a successful and brave man has dealt with the matter in that way, that men feeling that great amount of responsibility upon them would ever use that power when they have got what I consider to be a very much greater power in their possession—that is, good guns to make use of at a distance, and in the ordinary mode of warfare. However, perhaps that is an observation which I ought not to have made in discussing this paper, because that is a matter which is peculiarly within the province of the naval officer, and not of the constructor of vessels. I would like to say, just in further confirmation of this paper, that I entirely agree with the general character of the line taken by the paper as to meeting torpedo defence. I believe that torpedo defence must be met by auxiliary vessels acting in conjunction with these large vessels which we are now building and relying upon. What the size of these vessels that will accompany the fleet will be, is a matter of some considerable doubt at the present moment. Captain Noel has fixed it at about 1,000 to 1,200 tons, and though when acting in confined waters smaller vessels would be useful, yet in many seas these would be none too large. There is, I see, an interesting paper coming on immediately in this present session, in which M. Normand thinks that something like 50 tons displacement would be sufficient for vessels carrying out that work. I cannot at all agree to this. I think Captain Noel's figure is very much nearer the point than M. Normand's. I am quite sure that you do require, with vessels costing, as Sir Edward Reed has observed, over half a million of money, to have auxiliary vessels acting in conjunction with you that can, like scouts or like cavalry, come out and keep off such attacks as can be made upon you, and at the same time can possess great speed and that amount of gun-power that they can destroy vessels be they large or small—generally speaking, they will be small—so as to leave the larger ship in its more natural position of fighting vessels of its own size and vessels armed with great power. That, I think, is indispensable. I do not think that a torpedo

boat such as can be placed in the davits suitable for this service at all. I am speaking of vessels which are sufficiently large to keep the sea in company with the fleet, and of coming out and acting in conjunction with the fleet on those particular occasions. All that, I think, has been very admirably explained in this paper, and, without going into any other points, I think we ought to give the thanks of this Institution to Captain Noel for having given us the fighting views of the captains in connection with this matter.

Mr. N. Barnaby: I should like, my Lord, to say one word, or not much more, upon this paper. Sir Edward Reed has taken up the points which I had marked down here as being, as I thought, worthy of some remark, and I have scarcely anything to add upon those which he has touched, except that I would remind him—and he will be glad to be reminded of it—that there have been three or four cases of accidental ramming between ironclads belonging to Her Majesty's Navy, where ships have been struck and have not gone to the bottom. With regard to the strength of the outer bottom, Sir Edward Reed will remember what a keen discussion there was concerning the strength of the bottoms of some of the early ships that he has had in his mind. One of them in particular, members here will recollect, was the *Iron Duke*. One of the contentions of Sir Edward Reed was (and with that I always fully agreed) that the strength of the outer bottom in that ship was quite sufficient, and in fact she was a better ship than if she had been made stiffer. I do not see the late captain of the *Iron Duke* in the room, or I am sure he would be ready to confirm what I am going to say, that, unfortunately, the *Iron Duke* was badly ashore twice; that on one occasion she sued half her draught of water, and received no injury; that on the other she got upon the rocks and tore open the outer bottom, and that no water got into the ship at all from the inner bottom, except in one place where a stiff engineer's tube had been forced in through its gland, and the water found its way in by that passage. The outer bottom was torn open; the frames between the two bottoms were crushed, and there could not have been a better illustration, I think, than that ship furnished of the justice of the contention of the Admiralty officers in defending that class of ships. The question of the defence of that class is now a matter of history, but it is important as affecting our present practice, and I would refer therefore to one other ship about which we were told that she certainly was too slight—namely, the *Iris*. Unfortunately the *Iris* has been ashore quite recently, and the letters we received from the Dockyard officers at Malta, where she has been docked, are to the effect that the ship has behaved splendidly; that the outer bottom is considerably injured—or rather, I will not say considerably injured, but there are injuries to the outer bottom all along the length of the ship. I withdraw the word considerably, and say that the bottom is broken through to a certain extent, but that the defect can be made good very easily, and in a very short time, and the ship appears to have never been in the least danger of having water admitted into the ship proper—that is, inside the inner bottom. One other word as to "armoured tonnage," and the small vessels to which Captain Noel has referred, and about which Mr. Samuda has just spoken. We have had a great many discussions here of importance on this subject, and my own views are quite in accordance with theirs so far as they are worth anything. But there are very large demands made by Parliament—very rightly, no doubt—for armoured tonnage, and these vessels not being armoured, strictly speaking, do not count. So long as the strong demands are formulated for armoured tonnage, and the Navy Estimates remain where they are, the chance of getting these most important vessels is extremely remote.

Mr. W. H. White: My Lord, I wish first of all to join in the expression of thanks to Captain Noel for so fairly giving us his views. I would like to add, that I sincerely wish we had had the opportunity

of having one or more papers from other naval officers, because then we should have had illustrated very forcibly, what is the fact, that different naval officers have very different opinions on the same points as affecting the management and efficiency of ships. From the nature of the problem, as Sir Edward Reed has suggested, that is inevitable, and I think it is quite proper: but speaking generally, and confining myself to Captain Noel's opinion, I think, if he will excuse my saying so, that he has not fully considered the difference between what may be called the provision for the peace conditions of living in a ship, and the conditions of fighting her in action. Now, the designers of ships have to think of both. It is true that the more important is the condition for action, but the condition for action is one to be reached by preparation. To take an illustration, I think that scarcely any naval architect would put a water-tight door in a bulkhead if he thought that the ship could be worked without complaint without such a fitting; but, speaking now from an experience of many years, and I know I shall be very fully confirmed by everybody who has built war ships, the difficulty is to keep out water-tight doors. You start with what is considered the minimum number, the ship proceeds, and the tendency always is to increase the number. For instance, it is difficult to pass along at a certain level in the ship; the suggestion is made at once to put in a water-tight door. It can always be closed. The necessity for closing it in action will come once in twenty years, perhaps, but the desirability of having it there is of everyday occurrence. That is one out of many illustrations that might be given of the way in which the peace conditions of everyday service interfere with readiness for action. And if I may be allowed, just following up the remarks that have been made as to the complication of ships and the impossibility of overtaking that complication under the conditions of action, it seems to me, from my observations upon the subject, that there is always a pretty safe assurance that drill will come up to fittings. To take, again, water-tight doors, the *Vanguard* disaster was one which we have not altogether reason to regret. It is of course to be regretted, but it has its good side. I am speaking again as an observer merely, and Captain Noel will confirm me in this: the drill of the Navy has greatly improved from that time in closing water-tight doors quickly, dealing with large openings on the outside of the ship, and so on—all these things have received the greatest attention in the Service since, and the drill of the Navy has overtaken the necessities of the case. So I believe it will always be. Perhaps the designers of ships err in one direction; they may tend to greater details in fittings, greater complications, more mechanical refinements, than the sailor feels comfortable with —and I can only say I am very thankful I have not to manage a modern ironclad—but, at the same time, I think there is a rectification continually proceeding, that is to say, the instructed officers of the Navy are continually overtaking their responsibilities, and I believe they always will. It must be remembered that these fittings are always subject to naval criticism in all their stages. They are not put in at the whim of any ship designer simply; they are put in after conference between the designer of the ship and the naval officers. Captain Noel will be glad to hear that some of the recommendations which he makes in the paper are established practices in recent ships. For example, we will take this which he has mentioned, and to which he has very properly attached importance, the question of escapes from spaces which must be occupied in action. In modern ships these are provided. Perhaps it is possible to make it too easy for a man to run away. I do not know whether that is so, but at all events in all cases where good reason can be shown, and where it is possible to do so, the conditions which Captain Noel has formulated on page 5, as to providing for a man escaping from the torpedo compartments, the stokehold and so on, are very carefully considered, and almost on the lines he has indicated. I should like to add one word on the pilot tower question, if I may. It is to ask Captain Noel if he has in the least degree considered what weight would be involved in giving anything like efficient protection, that is, a decent thickness of armour, under many of the conditions where this conning-tower might

be needed. It is not merely the position in which the weight is to be carried, it is the question of the weight itself which is important often, and that is true, of course, in relation to the size of the ship and its other qualities. I am not expressing any personal opinion about conning-towers at all; I am not quite clear that I have one. I have many, it seems to me, in relation to the various classes of ships to which that protection should or should not be given. But although there is no standard pattern of conning-tower for all classes, it appears to me that it is always a question of the balance of advantage. Can you afford to give up the weight which would be required? and it will be a considerable weight, I think, with such a tower as this. Of course, many naval officers say, "We should be content with protection against machine-gun fire, we do not require armour at all." Others say, "We desire to have strong armour protection." Some again say, "We should like to go inside the towers," and others, "outside the towers." For those who actually have to determine whether they will put a tower or not in a particular class of ship, such a difference of opinion involves very great difficulties in decision. I do not think that Captain Noel, if he were to work this out with a 12-inch thickness of armour, for a common type of ship, such as is now built, would find that he would get this tower for less than twice as much as has ever been put into a pilot-tower yet. That may be quite a wise distribution of weight, but I would be glad to know if he has considered it from that point of view.

Vice-Admiral DE HORSEY: My Lord, I rise to contribute my thanks to Captain Noel for the very interesting lecture he has given us, and beg to be allowed to say a word or two with regard to Mr. White's last remark as to whether we should have armour-plated conning-towers or not. I think the majority of my brother officers will be very glad to have an armoured conning-tower if it can be given without introducing too much weight, and therefore without sacrificing other necessary things. I am of opinion, myself, that, viewing the great necessity for economising weight, which is so well known to Sir Edward Reed and others, the enormous weight, as Mr. White has pointed out, that is necessary to make an armoured conning-tower of the required size cannot well be afforded, and especially at so high a point. We know the necessity for keeping our weights down as low as possible in our armour-clad ships, which are all, more or less—and must be—top-heavy. I think that most naval officers (I for one, certainly) would be content with a conning-tower which is proof against machine guns. You are thus enabled to make it larger so as to give the necessary space for directing your guns, and your helm, and your torpedoes, but if you at all pinch the size of the conning-tower in order to economise weight, which a constructor would be very apt to do, then the conning-tower loses enormously in value. One word I should like to say, my Lord, with reference to Mr. Samuda's excellent exposition of ramming. I cannot agree with him (and I think the lecturer will be of my opinion) in thinking that ramming is not an important thing. I look upon it as a most important mode of attack, and I do not think Mr. Samuda could have put its importance better than when he so graphically explained to you how in a moment you could send 800 people to the bottom. I think you will find, my Lord, that no naval officer will hesitate for one moment, if he gets an opportunity, to avail himself of such an admirable mode of dealing with an enemy.

Admiral Sir COOPER KEY: Before Captain Noel replies upon this discussion, may I say one word which may perhaps help him in his answer. It is only on the important question of conning-towers. Mr. White asked the very difficult question, What would be the weight of the tower he proposes? I just wish to say that I do not think 12 inches of armour are nearly sufficient for a conning-tower. I think if you have only 12 inches you had better do away with it altogether. If

you are to have a secure conning-tower it should be at least 15 or 16 inches thick. I hope Captain Noel will give us the weight he considers necessary for a tower of that description.

Captain Noel: My Lord and gentlemen, I have to thank you very sincerely for the manner in which the paper has been received. I wish I could have put more value into it, but I was very much pushed for time. I was asked in January whether I would read a paper, and having a few notes with me, I put this together. I wish you to understand that in this paper I speak for myself as a naval officer, and not in any way in behalf of the Navy. In the discussion we have had some interesting points raised, and I had perhaps better go through them in order. First, as regards the strength of the ram. Sir Edward Reed brought forward a question with respect to the bow of the *König Wilhelm*, and, according to his statement, the armour in that case did not support the bow; but I think we may say that the bow of the *König Wilhelm* is one that has great weaknesses, one of a type not to be found in our service. Our bows are not so long, they do not project so far; such projection is alone a matter of weakness. The bow of the *König Wilhelm* extends very far from the main part of the ship; it is also a very narrow bow; it has no root, which I ventured to suggest was requisite; it is a sharp angular bow, which naturally gets very little support from the armour. I brought up the question of armour supporting the ram on the authority of the lamented friend of the Institution, Mr. Scott Russell, who talked of the bow being built of thick smooth plates, all supporting each other. But to get strength in the bow I take it you require a *root*, a firm structure on which to build your bow, which is in itself inflexible and cannot be easily turned; then on that root I fancy that armour would certainly be a help. As regards brass stems, I have met with much opposition on this question. It may be that I am not sufficiently well informed, but the idea that occurs to me is this: it is not a matter of the tensile strength of brass, it is the power it has to resist a sheering force. Now, in ramming, the ram point is generally made so as to strike a vessel below her line of armour, and the greatest strain will come on that part of the stem which takes the armour shelf, and that will be essentially a sheering strain. I rather incline to hold my point, that brass would not stand that sheering strain in the same manner as wrought iron or steel. As regards ramming, Admiral Tegetthoff was one of the greatest authorities on the subject. He was the first man in the field to show us what ramming could do. Naval officers, however, must not listen to Mr. Samuda, or rather to what Mr. Samuda has told us about Admiral Tegetthoff. They must not lose heart. The thing is, that if we do not ram we shall be rammed, and therefore in self-defence we must ram. And, as I have stated before, in a pamphlet I wrote some time ago, if you intend to ram your enemy and you keep your bows on to her and at the last moment flinch, she will ram you, unless she is badly managed; of course, on the other hand, if she flinches, you ram her. But the tactics of ramming are a science which I am certain (when we do have the war which must come some day) will be very important. Ramming is a thing that we cannot shirk, and for that reason our stems should be fit for the work. As regards water-tight compartments we shall hear more, I think, in the next paper. I quite agree with Mr. White, that it is very often the naval officer who wants the openings, but, all the same, they are most dangerous things. There is little doubt of this, that that very ship, the *König Wilhelm*, when she rammed and smashed her ram in, would have sunk had she not had an intact bulkhead forward; there happened to be an intact bulkhead some 50 feet from the bow, which kept the water from filling the ship. I am very pleased to hear that there are modes of escape from below; I did not doubt that the matter had been considered. I went over the *Polyphemus* one day, and I felt that I should not like to live in the stokehold; for when the ship was bumping about between ramming, sending off torpedoes, explosions occurring on board, and one thing and

another, he would be a plucky stoker who would stick to his work. As regards the conning-tower, I am exceedingly glad to find that it has created interest in the meeting. Conning-towers I have looked upon as one of the most important things in the fighting efficiency of a ship. I have compared a ship without an efficient conning-tower to a man without an efficient headpiece; I have not said efficient brains, because it might be remarked, "Oh, you mean that the captain represents the brains, and therefore you want to protect the captain." Now, it is not protection for the captain that I want. If the captain suffers there is another man to take his place, but if the communications suffer the ship is done for. It is the communications that we want to protect. The communications are the nerves, the ship being the body, and the protection of those communications is a vital necessity for the ship. Therefore I do not care what weight is given; let the weight be what it may, take that weight from the armour, or take it from the guns, take it from anything, but let us have the communications so protected that we can fight our ships. The *Huascar* had her tower knocked away the first thing, and there she was helpless in the action with the Chilian vessels.

Vice-Admiral De Horsey: It was an armour-clad vessel.

Captain Noel: She was armoured, but very poorly armoured. Still, the thing is to keep the communications clear and under good protection. Of course, viewing this plan, it looks as if the conning-tower is a very large business, but the lower part of the conning-tower need only be an armoured funnel or tube. This also must be borne in mind: that a structure of an oval or oblong shape need not be of such very great thickness. With all due deference to Sir Cooper Key, I think nine or ten inches in that shape is almost sufficient; at all events, I should limit it to twelve inches in most cases. Then comes another thing. If the position is well chosen—all these points are considerations for the designers of ships—the conning-tower should hardly be visible from outside. The bridge netting can be built in with the nettings of the ship. So long as the conning-tower looks over everything, that is all that is necessary. You do not see the conning-tower as a large tower standing up in the ship; you merely see it as shown in the plan, a foot and a half above the netting. The bridge netting would probably be a little above the ordinary netting of the ship, and other obstacles should be kept below that. Therefore, the conning-tower is not an object very easily observed from outside. As to torpedo defence, it is of course entirely a matter for the constructors, to decide how to get the necessary strength to resist torpedoes in the construction of the bottoms of ships; whether with weak outer bottoms and thick inner bottoms or *vice versâ*, or by some other plan. But I think there is very little question that, seeing how greatly torpedo attack has improved in every way, and how the attack by torpedoes is likely to be cultivated by almost every nation that has a seaboard, we should pay particular attention to the subject. I think that the meeting, as far as we have heard, agrees with the idea (which was not mine at all) of the necessity of having torpedo vessels of great speed, and I am very glad to be supported by Mr. Samuda as regards the tonnage; I do not think we shall get thoroughly efficient sea-going vessels very much under 1,000 tons. Mr. White has said a good deal about peace considerations. Now, I hold that war considerations are far more important than peace considerations, and I imagine that naval officers are willing to sacrifice a good deal to know that their ships are thoroughly efficient for being taken into action. I do not think I have anything more to add.

The President: I am quite sure you will allow me to convey to Captain Noel your united thanks for his most valuable paper, and to thank those gentlemen who joined in the discussion for their remarks. I am perfectly sure of this, gentlemen, that there is no class of criticism that the

designer, the naval architect, hails with more thorough satisfaction than naval criticism, because he brings his study and his science to bear upon the machine, but the naval officer uses it, and consequently it is well—it is essential, in fact—for the success of our shipbuilding, that these criticisms should be given, and it is upon that account that I am extremely gratified to-day to see so large and influential and able an attendance of naval officers. I only hope it is the beginning of better times, and we shall have more naval officers attending our sittings than we sometimes have had in the past. Perhaps, gentlemen, you will allow me to convey your cordial thanks to Captain Noel.

BULKHEADS.

By James Dunn, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 14th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

In a treatment of the subject of Bulkheads, I do not propose to deal with our men-of-war, because it is well known that the Admiralty has accepted the principle that for war purposes a ship should be subdivided into as large a number of water-tight compartments as is practicable; and to such an extent is this carried that in some of our armour-clads may be found upwards of one hundred separate and distinct spaces, a considerable number of which may be destroyed by the penetration of projectiles or by collision without the floating powers of the ship being seriously impaired.

I propose rather to deal with vessels of the mercantile marine, in which, besides being our chief source of commercial enterprise and wealth, the State itself is so largely and increasingly interested; and for this purpose I will submit three propositions for consideration:—

I. Is the subdivision of a merchant ship by water-tight bulkheads practicable, and consistent with commercial requirements?

II. Can these bulkheads be made sufficiently strong to withstand the pressure of water under all circumstances?

III. Are bulkheads of any value in securing floating powers for the ship in the event of damage from collision or other causes?

It may be thought that the order of these propositions might have been reversed, but the reason for placing them as I have done will become apparent.

I. In considering whether they are practicable and consistent with commercial success, I would prefer to give facts rather than opinions; but I may be permitted to say here that it has been my duty for several years to give special attention to this question for merchant ships, and I have had the opportunity and advantage of fairly considering it in its wide and several bearings in consultation with the shipowners.

In our early days of iron ships, we found them with a collision bulkhead placed at a few feet from the bow, and of great value it has proved.

At the stern also was a bulkhead, or a box, through which the shaft passed water-tight into the tunnel or passage; and enclosing the machinery and coal space, or money-spending department, were two iron bulkheads required by Act of Parliament, thus leaving two spaces, the fore and after holds, for the freight-carrying or money-earning branch.

At that time such an arrangement practically divided the ship into three equal lengths, which was thought very satisfactory. By and by shipowners' views expanded; they lengthened their ships, but reduced the machinery space by means of improved machinery, so that the cargo holds became each more than twice the length of the midship compartment. Then, *i.e.*, in 1862, that section of the Merchant Shipping Act was repealed which gave to the Board of Trade surveyors the power to require water-tight partitions to be fitted.

We cannot believe that this repeal was intended to encourage the idea that water-tight partitions were unnecessary, but rather that in large vessels more partitions than the provisions of the Act required were necessary to secure the safety of the ship, and it was doubtless thought better to leave builders and designers unfettered than to tie them down by general statutory regulations. Otherwise the State, in case of loss, might have been credited with the blame, on account of the deficient bulkhead arrangements authorised.

The spirit in which this view was accepted is indicated in the substance of this paper.

The partitioning off of the machinery space was, however, a necessity, and was continued, and in this position the Admiralty found the question, when, in 1875, the subject was investigated. They laid it down as an axiom, or rather reaffirmed it, that no ship should be considered suitable for purposes of the State without her bulkheads being so arranged that if any one of her compartments were laid open to the sea in smooth water the loss of buoyancy thereby occasioned should not endanger the safety of the ship; and after careful inquiry, the serious conclusion arrived at was, that there were not thirty British ocean-going ships in existence complying with this requirement, securing a minimum measure of safety.

The attention of the shipowners was then invited to the subject, and nobly have they responded, for at the present moment there are 300 such ships, including all the latest built first-class steamers, and many in the several other classes; and there are some now building which embody the all-important and additional advantage that they will be able to dispense with the buoyancy of *any two* compartments, and still retain sufficient floating power. While congratulating ourselves on having as many as 300 well-divided ships, we must not forget that there are more than 4,000 ships of 100 tons and upwards which would sink if any compartment between the collision and stuffing-box bulkheads were laid open to the sea in smooth water.

It is worthy of notice that the gentlemen forming the committee and staff of Lloyd's Registry have by their action shown their view of the importance of this question.

In 1880 they *recommended* for ships 280 feet long and upwards that a divisional bulkhead should be fitted in the forehold, extending to the main deck.

In December, 1881, the recommendation is omitted, and the fitting made compulsory.

In June, 1882, the divisional bulkhead in main hold is to be carried to the main or upper deck, and a corresponding bulkhead is to be fitted in the after hold in all ships 330 feet in length and upwards.

And they went further still, and required that in their one, two, and three-decked ships the bulkheads enclosing the machinery space should be carried water-tight to the upper deck.

This welcome progress we hope will go on, and that we shall some day learn that "spar deck" and "awning deck" steamers are to be similarly well treated.

And we shall hope, too, that the divisional bulkheads shall be fitted in all classes, and that they shall not, as they may now, stop at the deck which is awash, but that they shall be carried to a deck well above the load water line.

One important fact I will name before leaving this branch of the subject, and that is, that in no instance where an owner has introduced the additional bulkheads asked for by the Admiralty has he retreated, but rather has he advanced. And this, I submit, is an answer to be accepted to the proposition that bulkheads are practicable and consistent with commercial success.

II. We have now to consider as a second proposition whether bulkheads can reasonably, and with moderate weight of material, be made to withstand the extreme forces to which they may be exposed.

What are these forces ?

To consider the question fairly, we will assume one compartment laid open to the sea by the tearing of the side plating, and we shall have—

(1) The statical pressure due to the given depth of water in the hold when the ship is at rest and no cargo on board.

(2) That due to the pressure when the holds are wholly or partially filled with cargo, and the ship still at rest.

(3) That due to the extra pressure when the ship is under way, or alternately rising on the crest or falling to the hollow of a wave.

(4) That due to the rolling, pitching, and 'scending of the ship herself.

The first condition can readily at any time be tested, either by filling a compartment when a ship is on the blocks, and the test water can readily be cleared ; or it may be done afloat, when the pumping powers may be tested in the clearance of the water,

It will, perhaps, be said that this is not a test, that there are times when a much more severe strain is brought to bear, when the side is torn open, and the full force of the sea has to be met, or when the water is rushing to and fro, caused by the motion of the ship itself. I would submit that a parallel to even this test may be applied by providing a pipe with a head of water of, say, four to five feet above the water level; for I do not think that under any circumstances will the bulkhead be more severely tried.

When the hold is partly occupied by cargo, the volume of water is limited, its depth is lessened, and its movement is retarded.

In the case of the travelling ship, or the travelling wave, with its hollow and crest alternately rushing along the ruptured side, one might at first sight expect a great variation in the depth of water inside the ship, and a corresponding increased or lessened pressure, but it will be found in reality that the motion of the water outside does not indicate what is going on inside, as, through the largest cavity of which we have any knowledge, the water has not time to get in and out again with the speed of the passing wave; and the difference in the pressure on the bulkhead is consequently only that due to a not important variation in the depth, or it is a statical rather than a dynamical force. Where the hold is occupied by cargo, we have already said the motion of the water is stayed, and in spaces where the water is free, the motion of the ship having a varying period from the surrounding waves, the movement of the water is dilatory, and the pressure on the bulkhead varies very little and very slowly.

Much, of course, depends upon the position of the hole and the state of the weather. If at the broadside, and the water is smooth, the pressure is known. If in a seaway, the water runs past the hole, and little variation in pressure occurs. But if the hole is in the bows, the tendency is for the water to get heaped up in proportion to the speed at which the ship is travelling. Some interesting results illustrating this were obtained some time ago at the Admiralty experiment works at Torquay.

With the pitching and 'scending ship, extra forces are thrust on the bulkheads at the ends of the ship, and point to the necessity of making the end bulkheads, and especially the collision bulkhead, exceptionally strong; and it becomes the duty of the naval architect to consider the fact and prepare to meet it. How is this generally attempted? The very general practice is to adopt the rules laid down by Lloyd's Registry, which provide for plating $\frac{4}{16}$ths of an inch in thickness for a 1,000 ton ship, to $\frac{7}{16}$ths of an inch in thickness for the largest class. In the smaller ship the plating is stiffened with vertical angle bars, with flanges of 3 inches and $2\frac{1}{2}$ inches in width and $\frac{5}{16}$ths of an inch in thickness, placed 30 inches apart. And for the largest type of ship, with the thicker plating, these vertical stiffening bars are still placed 30 inches apart, but the flanges are each $4\frac{1}{2}$ inches wide, and their thickness is increased to ths of an inch.

Where a deck exists, it of course acts as a longitudinal stiffener or prop; and where the internal arrangements dispense with a deck, but where the distance between the

horizontal angle bar at the head of the bulkhead and the floor exceeds 8 feet, an angle bar equal to the main frame of the ship is riveted to the bulkhead on the opposite side to that on which the vertical stiffeners are placed, and arranged horizontally; and where this distance exceeds 12 feet, two such stiffeners are provided, and so on, the number of them being added to as the depth increases.

These arrangements, I submit, if efficiently carried out, should be sufficient to enable the bulkheads to hold their own in ships of the narrower type; and, as a fact, we know they have actually withstood the test under severe trials.

I may be asked to cite cases; some will be given later on in this paper, and the details of some others will be found in the paper which I had the honour of reading before this Institution last year.

We know, too, of many cases where the bulkheads have given way, but an examination would doubtless have disclosed neglect of the precautions to which I have referred; for too often have we found both horizontal and vertical stiffener cut for some insignificant purpose; the main prop of the bulkhead has been cast loose, and no wonder that in the hour of trial they have been found wanting, and the loss of the ship has been the consequence. While asserting the opinion that the rules referred to are in general sufficient to meet the case, we must remember that our merchant ships are increasing in breadth.

Three years ago 50 feet was a great beam, we have now an Atlantic liner with a beam of 57 feet; and the time has come for us to consider what additional means must be adopted to secure the safety of bulkheads. I would urge now for ships of great breadth, and for bulkheads of great area, that a vertical web-plate should be fitted at the middle line, say, from 12 to 24 inches in depth, with angle bar flanges, and secured to the bulkhead and to the several decks and the floors; and I shall hope to see some of the angle-bars between it and the sides of the ship replaced by good stiff bars of a Z section. I think we need not then fear to receive a good account of the ship when tested in this particular way.

Given, as I have before said, a bulkhead structurally arranged as described, well caulked and made water-tight to start with—a duty too often neglected—we have no fear but that it will be sufficiently strong to withstand the pressure of water to which, under the most extreme circumstances, it may be subjected.

III. I shall ask you to agree with me that a more general introduction of bulkheads is practicable, and that they can be made of sufficient strength, and I will now ask you to consider with me whether they are likely to be of any value for flotation purposes in the event of a ship having her bottom injured from collision or other causes.

We contend that they are useless if not wisely placed, nor carried high enough, nor efficiently cared for; they are useless when found, as we have found them, with stiffeners

cut, with rivets omitted, with caulking neglected, with plates removed, with large holes cut for small pipes to pass through, with sluice holes and no covers, with doors and worthless securities, or with open doors rusted and unmanageable, or with doors in the holds fastened open in such a way that they cannot be closed without "handling," and are out of reach at the moment of danger.

We will go further, and say that they are not only useless, but that under some circumstances they are positively dangerous. This may, perhaps, be thought a serious and startling assertion; but we will take the case of a ship illustrated by Fig. 1 (Plate I.) —and there are many such ships now afloat—in which a good number, a really large number of bulkheads, are provided and distributed as shown, but three of which, it will be seen, are stopped at the deck which is awash. The bottom gets damaged and springs a leak, say in No. 1 hold, or in No. 2 hold, or in both; and how many such cases have we known where the water enters and gains on the pumps, and slowly, but surely, rises to the top of the dwarf bulkhead, causing the ship to trim as indicated in Fig. 2 (Plate I.)

The water is then free to flow over the top of the bulkhead and pour into the next hold, the effect of which is inevitably to send her head-first to the bottom.

Now we assert that such a ship would keep afloat with the water in No. 1 hold *and* in No. 2 hold, provided it is confined by the bounding bulkheads being carried a few feet higher than the natural level. What this natural level is, and to what height the bulkhead should be carried, are points readily determined by the naval architect.

But if they are not carried up, but are left as shown,—and in too many cases they are so left—then we say they had better not be in the ship at all, as they will contribute to her loss by keeping the water at one end of the ship and carrying her bows under; whereas, if they are not fitted, the same volume of water entering as is indicated in the preceding diagram, and not being confined to one end, will distribute itself through the ship all fore and aft, in which case the trim is preserved, and she will still float in the position indicated in Fig. 3 (Plate I.)

Here, although the freeboard is reduced, she will still be seaworthy; the fires may be kept burning and the machinery going sufficiently long to bridge over the space dividing life from untimely death.

We will now take another view, and consider two cases, in one of which the bulkheads were well placed and cared for, and proved that under such conditions they may be of the greatest value; the other case is in all respects a contrast. In the first case they were placed in the positions and carried to the height indicated in Fig. 4 (Plate I.)

A steamer of nearly 5,000 tons ran into this ship in a fog, struck her abreast of No. 3 bulkhead, opening up two compartments to the sea; but, fortunately, the bulkheads had

been carried to a reasonable height, and the water could not get beyond them; they stood the test, she did not sink, but she kept afloat at the trim shown in Fig. 5 (Plate I.) and in this condition steamed 300 miles safely into port.

Happily, we are now getting a number of such ships, and many similar facts giving actual beneficial results might be placed before you if time would permit, so we will consider the next case, where we have the same number and a similar disposition of bulkheads as in the previous case; but, unfortunately, some of them are rendered valueless by being stopped at or about the water line, as indicated in Fig. 6 (Plate I.)

This sketch represents a large number of first-class steamers now afloat, and should such an accident happen to any of them as has just been described, they would certainly not have the good fortune to complete their journey, as in our last case; but the water, not being confined to the two holds numbered 2 and 3, as it was in the previous case—which is an actual one—will pour over the top of the dwarf bulkhead into the foremost hold, and the ship will soon get into the position indicated in Fig. 7 (Plate I.)

Water will then be reported to be making in the engine-room, if indeed she should not disappear before then.

By desire of Mr. Barnaby, one of our vice-presidents, and with the sanction of the Admiralty, two fair-sized models have been made to represent the merchant steamer of a very acceptable type, judging from the large number built and building.

In one of these models are placed the bulkheads according to a common practice, *i.e.*, with the two bulkheads enclosing the machinery space and a divisional bulkhead in the fore-hold.

In the other the bulkheads are arranged so as to represent a ship entitled to admission to what is known as the Admiralty List, *i.e.*, with four bulkheads beside those at the bow and stern, viz., two enclosing the machinery space, one divisional bulkhead in the fore-hold, and a corresponding one in the after-hold, all of which are carried to the upper deck.

Figs. B & G (Plate I.) will enable you to form an idea of the kind of ships (about 3,500 tons gross register) represented by the models.

I regret that it has not been convenient to bring the models and tank here, but I have permission to say that any member of this Institution may see them and operate with them at the Admiralty Office, 28, Spring Gardens. You might then judge for yourselves as to the advantages of good water-tight subdivision.

We have shown how bulkheads, when ignorantly treated, may be useless and even dangerous, but we maintain that when wisely dealt with they are of the utmost value, both from humane and economic considerations.

The models are loaded with weighted wood blocks, the blocks being of a bulk to represent the cargo in a passenger ship floating at an ordinary load draught with each compartment below the upper 'tween decks appropriated to cargo, having one-half its space occupied—a condition ordinarily assumed at the Admiralty when determining whether a ship is qualified for the Admiralty List—and they fairly represent such a ship as regards their measure of stability.

A hole is made through the bottom plating, to represent an actual hole, about one square foot in area and eight feet below the water surface in each compartment, and a plug is placed in it, so that by removing a plug any part of the model may be laid open to the water.

The first, which we will call B, or the badly bulkheaded model, very soon disappears after the withdrawal of any one of the plugs, because the water rushing in soon rises to the level of the water outside, and is then, or before then, free to flow over the top of the dwarf bulkhead into the adjoining hold.

Take, for example, the plug out of the bottom in way of No. 1 hold.

But if the corresponding hole in the good or G model is opened up, the water soon gets in and finds its level, but it is then confined between the bulkheads, and the model remains afloat in the position indicated in sketch G_1 (Plate I.)

Whatever experiment is made in this direction with the B model, the result is the same, viz., she goes down; so we will dismiss her from further consideration, and go back to the G model.

We have already shown her in position with the forward compartment filled in sketch G_1, and that sketch also represents the trim she would take if the damage were to occur in the second hold from forward instead of the first, because, although this No. 2 hold may be, and often is, the larger, it is nearer the centre of gravity of the water plane, the leverage is less, and the effect on the trim is modified.

Take another case, and open up both the forward holds, Nos. 1 and 2. Of course we expect that the ship will then go down, because the alteration of trim will be so great that the top of the boiler-room bulkhead, although carried to the upper deck, is dragged below water, and the engine-room becomes filled; and thus we have the forward *three* compartments full, which would undoubtedly sink her.

But suppose we keep the water out of the engine-room, which we can do by making water-tight the casing round the funnel and engine-room hatch to, say, eight feet above the deck. I will not affirm that this is practicable, nor will I ask you to do it, but it may be of interest to know that in smooth water the ship would have buoyancy and stability, even when in this damaged state, and would float, as indicated in sketch G_{11}.

Other cases of interest might be produced if the rules of this Institution with regard to time were transgressed, but I do not now purpose being a transgressor, so will ask you to accept my statement that the ship would behave in a similar way if the after compartments were laid open to the sea; that the ship would float with the buoyancy of the machinery compartment lost; and that she would also keep afloat with *two* of her cargo holds damaged, provided one is forward and the other aft. The freeboard certainly would be small, but under ordinary circumstances she would keep afloat in smooth water and have stability. And we must remember how many collisions occur in a fog, when the water is smooth; more, indeed, than happen in a heavy sea.

I do not propose, at this time and in this paper, to deal with the question of transverse stability in these particular circumstances, further than to state that in these models some attention has been paid to give them a measure of stability comparable with that possessed by a merchant ship carrying cargo in her holds and passengers in her 'tween decks, and that they keep upright and possess stability when partially and when wholly immersed.

I mention this fact here because it will perhaps be noticed that I have dealt with the transverse bulkhead question only, and have not advised fore and aft middle-line bulkheads, as they, by keeping the water on one side, will sooner bring the perhaps unsecured side-lights under water, may give the ship an unsafe heel, and accelerate her destruction. There really is no choice when we can secure a moderate change of trim by means of good transverse subdivision, or have the same change of trim and a considerable loss of stability produced by longitudinal bulkheads at twice the expense and weight in material, besides the inconvenience of stowing machinery and the like in the holds. There are, moreover, objections to the middle-line bulkhead on grounds of strength when placed at the ends of the ship; rather would I see it through the midship part, where the weights are heavy, and where then the maximum strains might receive their equivalent in strength.

This bulkhead question has so many phases that one is continually tempted to amplify, but I have endeavoured to look at the question from the point of view in which bulkheads give floating powers to a ship. That this is of importance must be admitted without the necessity of being reminded that in each of the two past years more than fifty steamers have sunk through collisions alone. And a consideration of the subject by this Institution will hasten on the claims of water-tight subdivision, and lead to the time when we shall not be satisfied to ask that the ship shall be safe with one compartment in communication with the sea, but that she shall be safe when struck on a bulkhead, throwing open two compartments.

As an illustration of the great general importance of the subject of bulkheads in merchant steamers, the following statistical details and deductions should be of interest. The advantages of good subdivision are broadly indicated in the annexed table :—

British Merchant Steamers.	Average number in existence during six years ended December, 1882.	Average annual loss from all causes during six years ended December, 1882.	Average loss per annum.
Ships qualified for the Admiralty list	157	$1\frac{5}{6}$	1 in 86
Ships not qualified for the Admiralty list ...	3,483	136	1 in 25

These figures are very significant. It appears from them that the chances of loss from any cause are nearly four times as great for a ship not constructed to qualify for the Admiralty List as for a ship entered on that List. This proportion is greatly due to the almost absolute immunity from loss by collision of ships on the List, for during the first four and a half years of its existence, not one ship was lost from it by collision, although a considerable number of the qualified ships had been in collision, and escaped foundering, on account of the safety afforded by their bulkheads. Within the last year, however, we have had six casualties to ships on the List, and among them was our only loss by collision. In that case, the whole of the ship—a small one—was flooded abaft the engine-room, the two after-holds being opened to the sea. This was a case such as we have no merchant steamers afloat capable of surviving.

The smallness of the loss from the Admiralty List may also be due, to some extent, to the ships thereon belonging to wealthier companies, being better found and manned, and being more employed on ocean routes, besides that they may less often carry dangerous cargoes, or be overloaded, than the smaller ships.

None of these circumstances greatly affect the fate of a ship after collision, nor do they, as we shall show, reduce the *a priori* risk of collision; but they supply almost certainly the reason why no ships have, in the last six years, been lost from the List by foundering or by fire.

During this time, the whole of the losses from the Admiralty List (11 in number) have been from drifting on rocks, or otherwise getting fixed on shore, with the solitary exception above quoted.

In the same period, 76 ships have been lost which had been offered for admission to the Admiralty List, but had not been found qualified: of these, 17, or $22\frac{1}{2}$ per cent., were lost by collision, and 10, or $13\frac{1}{4}$ per cent., were lost by foundering; most of the rest stranded or broke up on rocks.

That the general superior character of the ships on the List is of no value in reducing the risk of collision is shown by the following comparison.

We can prove that of the entire British mercantile fleet of steamers, about 1 per cent., without distinction, receive damage of a fatal character by collision during the year.

Of the number thus damaged, those on the List remain afloat, while those not on the List are lost.

This is deduced from the following figures:—Referring to the table given above, we will take only those cases of collision to ships on the List which would have proved fatal but for their compliance with Admiralty requirements. These are 9, or an average of $1\frac{1}{2}$ per year, giving $1\frac{1}{2}$ in 157, or 1 *per cent. of prevented fatal cases.*

Again, the average number of ships sunk by collision per year from the unqualified part of the fleet is 35, and the average annual record of the fleet for the six years is about 3,500, also giving 1 *per cent. of* (in this case) *fatal cases.*

Thus, our statement is substantiated, that, the risk of *fatal* collision is about 1 to 100, irrespective of the class of ship, and that ships on the Admiralty List enjoy almost absolute immunity from loss by this cause.

It is, therefore, proper to consider that the vessels on the List have no natural advantage with regard to their safety beyond that due to their bulkheads. Our object is to show that this simple advantage is of immense value, and the rational deduction from the above considerations would seem to be that, having regard to commercial and monetary interests, as well as to the preservation of life and property, a ship should be subdivided by as many transverse bulkheads as practicable, all carried high enough to be efficient in securing the good results above exemplified.

DISCUSSION.

Mr. J. D'Aguilar Samuda: My Lord, the significance of this paper can scarcely be over-rated when taken in conjunction with the observation which your Lordship had to make at the opening of the session to-day; and, I might say, with such other matters as have recently been put very prominently before the notice of the public. The most startling statement appears to be at page 22 —first, that when a proposal was laid down as a condition which should enable vessels to be taken up or be considered suitable for the purposes of the State, there were not found thirty British sea-going ocean ships which were capable of accepting it. A little further on we are informed that notwithstanding all the advantages which these vessels had for obtaining the highest class that they can from Lloyd's, and by getting that class they can induce people to put goods in their vessels as though they were really ships of the first class that could be obtained in the country, no less than 4,000 ships of 100 tons and upwards would sink if any one compartment between the collision bulkhead and the screw stuffing-boxes were laid open to the sea in smooth water. I myself am not one of those who regard either the action of Lloyd's, or the action of the Board of Trade, as desirable in dealing with the way in which the construction of steamers shall be carried out; but I do think, when they take upon themselves these functions, that we should not be able to have laid before us so startling a statement as that we have laid before us here; and when I take that statement in conjunction with the statement made by the President of the Board of Trade the other day to the deputation of ship-

owners who went up to him—that the loss of life from preventible causes has increased more than 50 per cent. within the last twelve months, that is to say, whereas for the previous five years 350 ships have been lost, and after that in 1882 no less than 550 ships, or thereabouts, have been lost—all of them from preventible causes—it does appear to me that the public have a right to expect from such Institutions as our own, that we should express a very strong opinion that something should be done to give the public the advantages of that which the naval architecture of the country is perfectly capable of giving them, if the shipowners are only desirous of purchasing it of them, that is to say, very much safer passenger-carrying ships than they have at the present moment. For this paper which we have before us I beg to tender my special thanks, and I am quite sure you will all agree with me in thanking Mr. Dunn for having laid the matter before us, at any rate, in a forcible manner. It ought not, and it cannot be disregarded now that it has been so pointedly put before us as this paper has succeeded in stating it. Speaking of it generally, the object is to point out, first, that there is not sufficient reason why additional bulkheads should not be put in ships; but if it had been proved that putting these bulkheads in ships really did detract in some degree from the cheap way in which passengers and cargoes could be carried, still I think we have a right to press on the shipowner that that economy should be given up, and that the public should be protected by having safer ships given them than those which they are in the habit of getting at this moment. Those are the sort of things which we are bound, it occurs to me, to put very heavy pressure on the public, through the influence of this Institution, to get for the nation generally. The Admiralty seem to have done their work admirably in this matter. They have laid down a certain set of conditions which, evidently, if we are to take the statement made in this paper, have shown themselves to be correctly and well designed; because the saving with regard to ships adopting their conditions has been quoted to us to have been at least four times as great as that which took place when the Admiralty conditions were not followed, so that we should be able to ensure at least that amount of advantage for the public. I do not mean to say I consider that every single thing suggested here with regard to bulkheads is all that it occurs to me ought to be done, at any rate in the best passenger ships, at this moment. I think we should go still further. I think that so many dreadful disasters have taken place lately with reference to loss of life at sea, by collision, that something even more than subdivision by bulkheads is necessary, and I cannot help thinking myself, without having worked out the matter, or gone into it practically to any greater extent beyond looking into it generally, that the time has now come at which the public ought to insist on having something like the same amount of security up the sides of the ship which they have in large ships lately obtained at the bottom of the ships, that is, having a longitudinal bulkhead carried along each side, so that when a collision takes place the interior of the ship should not be reached by the vessel striking her. I know I shall be met in the same way, as this paper shows us we have been met all the way through, by it being said that that will decrease the profitable employment of the ship; but that should not be accepted by us as a sufficient answer. We should say, if you are to carry the public you must either put your price at such a price as will enable you to give them a fair chance of their lives, or not put yourself forward to carry them at all. I therefore do think that the greatest possible advantage will come from having such a paper as this put before us, and I hope and believe it will lead up to inquiries generally, and a certain amount of knowledge being gained by the public of how unnecessarily they are exposed to risk, and that we shall have both Parliament and the public taking such steps as will ensure for the future some greater security for life—at any rate such an amount of security as we have a right to get when life has been so severely sacrificed as it evidently has been, and where the causes are entirely preventible.

Mr. B. Martell: My Lord, I desire in the first place to express the great gratification I feel, and also the gratitude of the shipowning community at large, which I am sure they will feel, at the action taken by the Admiralty with regard to this subject. They seem to act, I am glad to say, on an entirely different principle from certain other departments of the Government. The Admiralty endeavour to assist the shipowning public, and the mercantile shipowning public has been assisted lately by the Admiralty in many ways. They have employed their staff in making calculations of the stability of mercantile ships under different conditions, and they have done all they possibly can in assisting in perfecting the mercantile ships of this country, and very great thanks are due to the Admiralty for the action which they have taken in this matter. Now, this paper is a very valuable paper, no one can question, and I quite agree that the Admiralty, in following out what was commenced by them some few years ago, in supplying the Mercantile Marine with all subjects within their knowledge affecting mercantile ships, are doing good and useful service to the Mercantile Marine, and are certainly deserving of their best thanks. Now, having paid this tribute to the Admiralty, which I consider to be really their due, I must at the same time say that I was quite prepared to hear the remarks made by Mr. Samuda. There is never a case which comes forward that can in any way be used as a peg on which to hang any remarks derogatory to Lloyd's Register of Shipping, but he is delighted to rush forward immediately and haul in Lloyd's Register. What has Lloyd's to do with this?

The President: Do not let us quarrel.

Mr. Martell: I beg your Lordship's pardon, but I felt it necessary to say this, because I was prepared to hear this, and have heard just what I expected to hear. Now, a man who puts himself forward as a representative of the mercantile shipping of the country should be a man who is in the habit of building mercantile ships for general purposes of trade, and, therefore, I do not think from the remarks which Mr. Samuda has made he is a fitting representative of the general shipbuilding public, and neither, it is evident, is he acquainted with what Lloyd's Register Society really require or what is usually done in constructing mercantile ships for general cargo-carrying purposes. In looking through the illustrations, one instance is given here, in Fig. 5 (Plate I.), of a ship crushed in. According to the present rule of Lloyd's Register, that ship would have the same number of bulkheads carried up in the same way in which they are here fitted as required by the Admiralty, and that are condemned in such a wholesale manner by Mr. Samuda. The rule for Lloyd's I can hardly expect Mr. Samuda to be acquainted with, because he builds so few cargo-carrying mercantile ships for general purposes of trade, that probably he has enough to do without studying Lloyd's Rules. However, it is this: "Screw-propelled vessels, in addition to the engine-room bulkheads, to have a water-tight bulkhead built at a reasonable distance from each end of the vessel. In steamers 280 feet long and above, an additional bulkhead is to be fitted in the main hold about midway between the collision and engine-room bulkheads, and extending to the upper deck in vessels with one, two, or three decks, and to the spar deck in spar-decked vessels, and to the main deck in awning-decked vessels; and in steamers of 330 feet long and above, an additional bulkhead is to be fitted in the after-hold extending to the same height." That is the requirement at the present time of the society which is condemned so much by Mr. Samuda, because the ships built according to the Rules of that society are so very unsafe, that shipbuilders would build much safer ships if they had nothing to do with Lloyd's Register Society at all. I would say with regard to the comparison which is made by Mr. Dunn, that it does not convey, in many respects, a proper meaning. It is accurate in itself, I admit. I should be sorry, of course, to infer for a moment that Mr. Dunn

in getting out this would not put precise and accurate information before this Institution; but we must bear in mind that nothing is so misleading as percentages. The ships placed on the Admiralty List—the great proportion—do not include the general cargo-carrying ships of this country. They are mostly the large passenger ships; ships that are specially protected by the owners, owing to the large number of passengers they carry, and therefore different from the general cargo-carrying ships. If you take the illustration that is given there, and consider that as a ship for general cargo-carrying purposes, no shipowners would think for a moment of dividing that ship into such a large number of compartments. I do not say that it would not be a good thing, but it would be perfectly impracticable, the same as it would be perfectly impracticable for general commercial purposes to fit in all cases a middle line bulkhead. As I understand, out of the whole proportion Mr. Dunn deduces there is 4 per cent. of losses in the generality of ships above 100 tons that are built, and 1 per cent. for the ships qualified for the Admiralty List. That is no fair comparison as far as percentages, because the number of ships put upon the Admiralty List bear a very small comparison with the general cargo-carrying steamers throughout the country, and if you draw a comparison embodying the whole number, you would find the percentage very materially altered indeed. I have no wish to extend my remarks any further than to say that this matter of bulkheads is a very important matter. But still, at the same time, I may say with regard to steamships, it is the fact of their carrying large numbers of passengers which makes it so important, as in sailing ships of 300 feet long, such as are now sometimes built, they have only two bulkheads, and it is not thought necessary to add more. If we are to go to the extreme limit, and make ships perfectly safe under all conditions, then we must go to a greater extent, in these large sailing ships of 300 feet long, by putting in more bulkheads than we have at this time. The only thing is, whether the shipowning community of the country will submit to this or not. I do not want to say a word against the fitting of any number of bulkheads, but what I do say is, that the thing must be looked at to some extent from a commercial point of view to see the actual necessity, from losses, for increasing the number of these bulkheads in sailing ships beyond what is done in the best class which are at present built.

Mr. N. Barnaby: My Lord, I have been connected with this Institution now a great many years, and I remember that one of the acts of the Council of the Institution in 1867 was to organise, under our late President, a deputation to wait upon the then President of the Board of Trade—the Duke of Richmond—and to represent to him that we, the Council of this Institution, were not satisfied with the way in which iron ships were being built, inasmuch as there was not enough security given for the safety of a ship in the event of a collision. Nothing came of that. Mr. Dunn has said that when this question was taken up by the desire of the late Mr. Ward Hunt, we were not greatly astonished, although we were very much grieved, to find how few ships there were that were divided in the way that has been described, and it is due perhaps to the firm which had done most good work during the period when the ships had been building, which we then had to examine, to say that one firm, and that an Irish firm, had done better than any other firm we could find in putting bulkheads into ships without being thanked for it by anybody. That firm is the firm of Messrs. Harland & Wolff, of Belfast. Mr. Martell has said that sailing-ships have no bulkheads, except collision bulkheads, and when we remember that the shipping which has been dealt with by Mr. Dunn is purely steam shipping, and that there remains the tonnage of ocean-going ships, ships of 100 tons and upwards on the British Register, as great outside the Steamship List as in it, that remark of Mr. Martell's is a very serious one. It must be borne in mind that iron sailing-ships, having no bulkheads practically, excepting the

collision bulkheads, are used for taking emigrants in considerable numbers across the seas. These statements, therefore, which have been laid before you, and which have aroused the generous indignation of our respected Treasurer, Mr. Samuda, bad as they are, are nothing to those which might be made, or which may be summed up in a very few words concerning the ships built of iron which have no steam power in them. It is perfectly understood by this meeting that the Admiralty have not taken up this matter from any humane motive: we do not go in for humanity. Our motive has been simply this, that we have seen in the immense merchant shipping of England —the steamship with its great strength of ship, with its high speed, and with its power of using modern armaments—the means of making the country far stronger in the way of national defence, but we had to say at once what we have said all along, when the question had ever been raised as to the use of such ships, "They will not bear touching; if you strike them under the water they always go down." But then we said to ourselves, "That is not necessary; surely the ships may be so bulkheaded that they may be safe when one of their compartments is full of water;" and I have nothing but praise for the shipowners, and builders too, with whom we have been in communication from the time we commenced the inquiry, nor have I anything but praise for the Committee of Lloyd's Registry, because they have always given us the strongest possible encouragement in what we are doing, and I have regretted to hear some things which Mr. Martell has said, because he does not do himself justice, or the Committee of Lloyd's Registry justice. I do not think that Lloyd's Registration Committee is put on its defence; that is my view. Then with regard to the Board of Trade, it is quite possible that we may hear some one from the Board of Trade speak this morning. We have been in communication with them, and we have found them exceedingly courteous and always desirous to render us any assistance they could, short of this—that they were not prepared themselves to lay down any regulations for making ships safer by putting in more bulkheads. Our difficulty on starting was this, that we were told by people who knew, that it was not possible to work ships if bulkheads were put in to satisfy that condition. Mr. Dunn's paper this morning shows that that is a fallacy—that the ships can be worked; and the diagram which is on the wall is not a diagram invented by Mr. Dunn to show a wonderful division such as he would like, but is an actual ship proposed by practical owners for the legitimate purposes of trade; and I am very happy to see that division is carried so far as we there see it to be. I should like to add one further remark, and to put before you a little note which we have. Confining ourselves to high-speed ships, that is, ships of 11 knots' ocean-speed and upwards, the number lost since December, 1876, has been thirty-six; the total number of British steamships—that is, of 11 knots' ocean-speeds, and upwards—lost since December, 1876, has been thirty-six. Of these, thirty had been offered for the Admiralty List, but only four had been accepted as being reasonably divided into compartments. Of the thirty-two not on the List ten are known to have been lost by collision, six by springing a leak, and four from causes not ascertained. Of the four which were accepted as being reasonably divided, they were all lost by going ashore. I do not think I have anything which I can usefully add at this moment to the statements which Mr. Dunn has put before you. We have long thought of making these statements public, but our difficulty has been that we did not wish to injure the owners of ships who had allowed us to inspect their ships, and therefore, although these cases which have been put before you this morning are really actual cases—if you could cross-examine Mr. Dunn and he chose to be confidential, he could give you the names of the ships he has dealt with—he has carefully disguised them, and the ships, as you see them in these diagrams, are not exactly like the ships in question in external appearance. But you may be sure of this, that what we have done here we have done with the most sincere admiration of the shipbuilding industry of this country, and the most hearty desire to do all we can to

help it, and also with the most hearty desire to get out of it as much as we possibly can for the defence of the country.

Mr. John Corry: I have listened with pleasure to Mr. Dunn's paper, but I shall not detain this meeting by going into the details of the paper; I will merely refer to one point, the construction of the transverse bulkheads in large ships. It is right and proper that ships should be subdivided, but it is most important that the subdivisions should be equal to their work, otherwise they are really of no practical value. In the very large and wide steamers which are now being constructed, I do not think any practical man here would like to risk his life on board one of them when some of the larger compartments were disabled, especially if she were some distance at sea. I think he would have very great fear that the bulkhead would give way, and of course the advantage then would be lost. Mr. Dunn has referred to special means of strengthening these bulkheads by adding web-frames some 12 inches wide; I think 12 feet would answer the purpose much more effectually. I believe, instead of attempting to strengthen bulkheads from side to side, a much more practical, simple, and economical way is to put partial bulkheads in the line of the stanchions, either single or double stanchions, as the case may be, from deck to kelsons, as by so doing you reduce the width of the bulkhead to one-half or one-third, as the case may be; and we all know that would strengthen it enormously. There is another great advantage in a practical point of view, and that is, that any strengthening in the middle line, or line of the stanchions, in no way interferes with the carrying capacity of the vessel or the stowage of cargo, which are very important considerations in a mercantile point of view.

Mr. Raylton Dixon: My Lord and gentlemen, I am not going at all to enter into the discussion which has taken place on the difference of opinion between Mr. Samuda and Mr. Martell, because I feel that if I did I should differ from both, and be only complicating it by making it into a sort of triangular duel. But, my Lord, with regard to some of the remarks which fell from Mr. Samuda as to the protection of life on board of steamers by this greater subdivision into water-tight spaces, I must say that I do not think he has sufficiently considered the peculiar circumstances under which cargo steamers are built; and as one having some experience in the building of this class of vessels, I think it is hardly fair or reasonable to speak of its being probable, or even advisable, for all vessels to be constructed not only with double bottoms, but with a double skin to make them so additionally safe. Mr. Barnaby has stated that the Admiralty do not go in for humanity, but I claim on behalf of the builders of commercial steamers that we do go in for humanity to the greatest possible extent that we can. Yet, my Lord, I think you will agree with me that there is always a certain limit to this, and that we can only go in for humanity so far as it can be introduced consistently with the carrying out of the duty which the structure has to perform. There is a certain limit. If we like to complicate the thing by simply looking to the question of humanity and to the safety of ships, we may make our ships so that they will do nothing, and will not perform the purpose for which they are created. As speaking more especially of cargo vessels we must consider as one of the most important items the question of stowage of the cargo, not only as to getting it in and out, but in and out with a reasonable amount of facility. I should like to ask Mr. Dunn how he is going, with a steamer like the one before us, to load it with a cargo composed say, in a great measure of 40 feet steel rails or large girder work, which is such an important part of the cargoes from that part of the country to which I belong? I think Mr. Samuda has put it rather unfairly on the builders of this class of steamer.

Mr. Samuda: My Lord, may I explain? I think Mr. Dixon did not hear what I said. The

observation of mine which he has commented on had reference to first-class steamers for passenger service.

Mr. Dixon: Then I quite misunderstood you, and I withdraw what I said. I thought you spoke of all vessels. I thought, not only in Mr. Samuda's remarks but in the paper of Mr. Dunn and the other remarks we have heard, that this question of stowage was not sufficiently considered, and if a rule were made absolute by Lloyd's, or any authority like that, under which most of these vessels have to be built, for still further subdividing the vessel, without considering the cargoes, we should make many of these ships perfectly useless for the trade for which they are intended; and the only way in which the matter can be adjusted or considered is, by shipowners in a great measure giving their attention to it, and considering how far they can possibly meet the matter by allowing their vessels to be built with a greater number of subdivisions and dividing them into smaller spaces. It is a question to be considered in connection with the several trades the vessels are constructed for; and it is impossible, such being the case, to lay down any definite rule, or any rule like the rule of Lloyd's, to compel builders to divide their ships into a certain number of compartments. I think that this question has hardly been sufficiently considered, or that there has been a sufficient amount of weight given to it. I can quite understand that Mr. Dunn and Mr. Samuda, having had to do with these large passenger vessels and other first-class ships, have not considered it as affecting that far larger class of steamers, those more subject to the danger, and that is, the cargo-carrying steamers.

Mr. E. Withy: I do not rise to make any remark upon this paper, but at the close of page 27 there is an offer to the members of the Institution that they may, if they like, go and inspect the models. I, and a gentlemen who is sitting by me, think that that would be most valuable, if Mr. Dunn could arrange a time to attend personally and show them to us. I have seen indications from shipowners themselves showing rather more inclination to divide the holds by thwart-ship bulkheads, and also to carry fore and aft iron divisions down to the kelson, than there was some time ago; and I am not sure that a view of the models after this discussion may not be very opportune. I mean, of course, at some hour at which the Institution is not sitting.

The President: I quite agree with that suggestion.

Mr. J. Harvard Biles: I should like to say, in connection with what Mr. Raylton Dixon has remarked as to that ship (Fig. G, Plate I.), as I have been intimately associated with the designing of similar ships under the direction of Messrs. Thomson, that in the holds, which may be numbered, the third from forward and the second from aft, that it is not at all difficult to get 40 feet rails down into those holds. And, further, I should like to say that in connection with the designing of some steamers, which are purely cargo-carrying steamers, we have devised arrangements whereby logs of timber, which are more difficult to deal with than 40 feet rails, may be shipped into a hold which is divided by a bulkhead, the hold itself being barely long enough to carry the log. The means of doing that is this: in the part of the bulkhead in the vicinity of the hatch —like the second bulkhead there, dividing the hold—the two hatches are common to the one hold, and that part of the bulkhead which extends up into the hatches is made portable and can be easily replaced, so that even longer rails than those which Mr. Raylton Dixon has spoken of might be carried in holds of that kind if small details, such as the portability of a portion of the bulkhead, were taken into consideration. Probably this is a thing which has occurred to a great many people, but I think it proper to make that observation after what Mr. Dixon has said.

Mr. Dixon: I quite agree; I withdraw everything I said if the bulkheads are portable, but I was speaking of what I understood were permanent bulkheads.

Mr. Biles: Allow me to say that that ship which has no part of her bulkheads portable would carry the 40 feet rails which I have mentioned. The hatches on the lower deck, of course, would have to be made much larger than those on the main deck; but still it is quite practicable to carry 40 feet rails in holds of that kind. Then there is a question of the structural strength which bulkheads add to a ship. That is a question which, I think, Mr. Martell and Lloyd's Committee perhaps have given consideration to, but which they have not put in their Rules in any way. The reason I mention that is this, that cases have come under our consideration in which we have proposed a modification of the size of the beams which principally add to the transverse strength of the ship in consideration of putting additional bulkheads in the ship. I think that water-tight bulkheads do add sufficiently to the transverse structural strength of the ship to enable shipbuilders and owners to reduce the strength of the beams. We proposed this to Lloyd's, starting on the basis of a certain sum of money. We proposed to take out a certain sum of money from the beams and put it into the bulkheads, thereby increasing the water-tight subdivisions. Lloyd's at present have not seen their way to accepting this subdivision, but I think it would be well if they were to consider the question with a view to doing something in that direction. Then Mr. Dunn in part of his paper says, "The bottom gets damaged and springs a leak, say in No. 1 hold, or in No. 2 hold, or in both, and how many such cases have we known where the water enters and gains on the pumps." In connection with this question of pumping, which is really part of the question of bulkheads, there is one thing which came prominently under my notice recently, that is, that although you may have large pumping power in a ship it is not always possible for the water that gets in the holds to get to those pumps. There are certain precautions necessary in order to keep certain cargoes from the bilges of steamers. The ceiling has to be made to some extent water-tight. These precautions which have to be taken, at the same time prevent the water which would get into the hold in case of leakage from getting to the pumps. This is a question which in many ships certainly might be fatal to them, and a question which, perhaps, I am speaking without sufficient knowledge of; but I have taken some considerable care to investigate it, and I think it is one which members of this Institution might do well to devote their attention to. Summarising it, it is this, that although you put a large amount of pumping power in a ship, there are certain difficulties which arise in connection with getting out the water which may enter a ship above the floors, which make it almost necessary, if you wish to have proper security in a ship, that you should subdivide her; that is to say, it is almost impossible in connection with considerations of economically carrying cargo to make any use of the pumping, and therefore you are forced to the necessity, if you have to provide for safety, of having much more subdivision than is generally carried out. The only security against accidents is subdividing, and not large pumping-power. Mr. White last year propounded the theory that pumps were not of much use, and I am merely following out to this extent the lines I believe he had in his mind. It was thought a great heresy, but I think if the facts he stated and the idea I have just mentioned are taken in conjunction it will be seen that what he said then is true, that pumping power is really of not much use to a ship. One idea more, which is in connection with the paper which I shall have the honour of reading before this Institution to-morrow, and that is, the advantage which arises in connection with increased beam. Increased beam allows you to increase the depth of a ship; increase of depth means increase of capacity; increase of capacity takes from a shipowner the pressing necessity of providing in many cases for efficiently stowing the cargo of his ship by taking out the bulkheads, and there-

fore, perhaps in a far-fetched way, though I think in a real way, the increase in the beam of a ship adds very much to the safety of a ship, by allowing a shipowner to increase the number of bulkheads by reason of the very much increased internal capacity he has. That is to some extent counteracted by the fact that he has to pay for additional tonnage, but that is a thing which he may be prepared to pay for, and is only incidental to the present tonnage laws.

Mr. W. John: My Lord, I will only occupy a few minutes. It seems to me that this paper, and the discussion upon it, have followed very much the course that many other discussions on bulkheads have at these meetings. I think the paper, and the discussion also, are calculated to do very great good. I do not think it will do much good to shipbuilders, and I do not think it will do much good to Lloyd's Registry of Shipping, or to the Board of Trade; but I think it will have a good effect in educating shipowners, because, after all, shipowners are the key of the position in this respect. I quite agree with the necessity of subdividing all these passenger steamers, and I would go so far as to say that the Government ought to insist on their being subdivided where so many lives are at stake. But when with that you mix up cargo steamers, you go so far beyond the line of what is commercial reason and common sense that you simply cannot carry it into practice. Now, I put it in this way, as I think I have before. You had in the mercantile marine, in the old days of wooden vessels, practically no subdivision. You came to iron vessels, and you followed the same lines, and the same ships in the mercantile marine have simply one bulkhead. That protects them from collision; and to go beyond that, and compel a shipowner to put in more bulkheads would be simply interfering with a class of ships which is insured at a lower rate than any other class of ships afloat, namely, the first-class iron sailing ships; and you cannot interfere with steamers without interfering with sailing ships on any reasonable consideration whatever, because, as a matter of fact, they are subdivided at present by two bulkheads at least, some of them by three. As long as the matter is kept to a question of life, then by all means subdivide. Mr. Barnaby has rightly pointed out the fact that some of these sailing ships carry emigrants, hundreds of them. I know, and we have heard, of sad cases of collision. Take the case of the *Northfleet*, which was run into. She was a wooden vessel. The Government could not well have insisted that those people should not go out to New Zealand in a ship of that class, which had been carrying emigrants for years. The shipowner, if he is to subdivide his ship, has to make her practically worthless, except for that voyage, and to take out all the work which has been put in her. And who is to reimburse him? As to passenger steamers, I can quite understand that public opinion acts more quickly on the owners; and the Admiralty, by the judicious efforts they have made in the direction of getting these vessels on the Admiralty List, are doing good work. If the Admiralty, or any other power, could act as effectively on the Underwriters at Lloyd's, and induce them to take into consideration the question of loss as affected by bulkheads, it might have a similar effect in inducing the owners of cargo vessels to subdivide. Until there is some inducement of that kind I fear that our discussions will not carry the matter beyond the stage of passenger steamers.

Mr. Maginnis: I should like, on this question of bulkheads, to lay before the meeting one matter that has not been touched upon, and that is, that no subdivision in merchant ships carrying certain cargoes (as very recently practical experience has pointed out to me) will do away with the risk of foundering, once the outer shell is pierced. The bulkhead division has been largely advocated for passenger steamers, but it is well known that there are at present no large ocean passenger steamers that do not carry cargo. To illustrate a danger to the best bulkhead subdivision that has not been broached to-day, I may allude to an accident which happened very recently to a steel steamer, over 400 ft. long, built by the firm that Mr. Barnaby said

had, without any intervention from the Admiralty or otherwise, always carried out proper bulkhead subdivision. [Mr. BARNABY: Not always.] Well, as a rule, carried out proper bulkhead division. This steamer, in coming out of a port in India a short time ago, struck a rock, owing to the slight upward and downward motion caused by the swell, making a hole about 3 ft. by 2 ft., just forward of the after bulkhead. I may mention that the ship was very nearly divided just as that sketch represents, the after bulkhead being a few feet further aft. This would have been no disadvantage to this ship with ordinary bulkheads, for it was found that all was tight except the one compartment, and the ship in perfect order, but before ten minutes were over it was found that the cargo in No. 6, as it was called, was swelling to such an extent as to force the tunnel and also the bulkhead between No. 5 and No. 6 holds out of position. The result immediately was that No. 5 hold then began to fill, not only round the portions that were broken that were about the tunnel, as was afterwards discovered, but also in various parts of the bulkhead. Then, again, as soon as the water found its way into No. 5, very visible signs began to show themselves of the enormous strain that had been put upon these bulkheads, as the next compartment to No. 5 was the engine-room, and in a short time the engine-room bulkhead was found also to be yielding, not from the pressure of the water, but through the swelling of the cargo, and the result was that stanchions, and such like, of 5½ inches in diameter were broken; but, fortunately, the engine-room bulkhead yielded enough to relieve this pressure, and the ship floated with the two after-compartments full of water up to the main deck. This steamer steamed 90 miles in this condition, with her main deck immersed aft, before she arrived in another harbour that could take her in. I think that this water-tight subdivision is certainly a splendid thing, but there is no doubt about it that for carrying grain and such like cargoes we can build no bulkheads, and we can put no stiffening into ships, that will stand the enormous pressure of such cargoes swelling. I think this point is one which is almost unknown (perhaps I am speaking rather rashly) to the Admiralty, who have so much experience in this bulkhead work, but, judging from what I have seen of the pressure put on that ship, I should think few boilers would bear the strain put upon those bulkheads. I may mention another instance. At the entrance to the Mersey recently a large Atlantic steamer was run into and sunk. That steamer, at her port of call, had a draught of water, with her 'tween decks, and all loaded, of about 22 feet, and after being beached in the Mersey, and all the cargo taken out of her 'tween decks—she had peas and other absorbing cargo in her holds—that ship when again floated drew 25 feet of water, notwithstanding that upwards of 1,000 tons of cargo had been discharged and nearly all bilge-water pumped out. I should like this danger to be publicly stated, because when fatal collisions occur, immediately the newspapers of every description, scientific and otherwise, pitch into shipowners for a bad distribution of bulkheads. I may say that at the present time the large Atlantic steamers sailing from the Mersey have, as a rule, enough bulkheads that would be required for any margin of safety until we find some way of relieving the pressure of the swelling of the cargo.

The PRESIDENT: Gentlemen, I rise with the greatest unwillingness to make an appeal to you. To-day is a peculiar day, the hall being required by the Society. I must, therefore, call upon you to bring this discussion to a close, important as it is.

Mr. DUNN: My Lord, I am sorry that Mr. Martell was not here to hear the earlier part of the paper. I tried to explain that we must not expect because ships had bulkheads they would not get into collision. We know as a fact that the ships on the Admiralty List do get into trouble in the same way that ships do which are not qualified for that List. I wanted to say this, that if a ship on the List gets into collision we have proof that she stands a very good chance of keeping afloat, and

that if she has not been subdivided into water-tight compartments she must certainly go down. Mr. Martell refers to Fig. 5 (Plate I.), and says that it is the division that they are now insisting upon at Lloyd's. I know it is ; and he might have heard me congratulate Lloyd's Registry on that fact. That is the only division we ask for in cargo ships. In reply to Mr. Raylton Dixon, I would remind him that dividing the ship in the way we ask would divide her into five compartments, and if we take a ship only 200 feet long, you then have each compartment nearly 40 ft. in length, long enough for general purposes, but possibly not long enough for some special purposes. We know there is a difficulty in subdividing a cargo ship. We know there is not so great a difficulty in subdividing the first-class passenger steamers. I have advisedly dealt here generally with the cargo ship, and have not particularised any cases, but within the last few days we have had sent to the Admiralty a sketch, showing how it was proposed to build a mail steamer for service on the Atlantic. I asked permission to show that sketch ; and that is it referred to by Mr. Barnaby. That diagram illustrates the arrangement actually proposed, not by the Admiralty, but by the ship-owner as to dividing a ship. The long black vertical lines represent the transverse water-tight bulkheads. The lower black horizontal line represents the water-tight deck. The short vertical black line represents the trunk connecting that main water-tight deck with the upper deck. A better division than that we do not wish, and if you will only give us that we shall be satisfied. I shall be very glad to show Mr. Withy, or any gentleman connected with this Institution, the model experiments at 28, Spring Gardens, any day between twelve and four o'clock.

The President : I am sure you will all wish me to convey our thanks to Mr. Dunn for his very valuable paper. I will only say that I do not think any paper could by possibility be better timed than this, because the public mind is so terribly excited with the loss of the *City of Brussels*, in the Mersey, the *Cimbria*, and many other vessels which have come before us quite recently. I am sure I may thank Mr. Dunn cordially for his valuable paper, and I hope the discussion that has ensued upon it will have a good effect.

EFFICIENCY OF GUIDE-BLADE PROPELLERS.

By J. I. Thornycroft, Esq., Member of Council.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 14th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

During the years 1879 and 1880 I made experiments with guide-blade and other propellers, using models of small dimensions.

These models indicated some advantages to be derived from the use of guide-blade propellers, and my firm has since fitted H.M. torpedo vessel *Lightning* with a propeller of that kind, and built a shallow steamer for the Congo, with a hull specially formed to suit the requirements of the propeller when used for very shallow draught. I now propose to give a short account of the results obtained with the models, and afterwards with the propellers as fitted to the vessels before named.

The models used were from 5 to 11 inches diameter, and were adapted to use from $\frac{1}{4}$ to 1 horse-power, at a speed of $4\frac{1}{2}$ knots.

In order to experiment with these models, a launch was fitted with a small shaft projecting directly forward from the bow into water which might be considered as almost undisturbed by the motion of the launch through the water, and the small shaft was driven by an engine with suitable gearing, to allow the turning moment exerted on the shaft to be continuously recorded, the shaft at the same time being free to move lengthwise a short distance, without hindrance, and allow the thrust of the propeller to be measured simultaneously.

The launch was propelled principally by another engine, driving a screw at the stern, and the speed of this latter was found to measure the speed of the launch very nearly. A distance of 300 feet was measured on the bank of the river; the time running this distance, the revolutions of the main and experimental propellers, the turning moment and thrust of the model were all recorded on a sheet of paper held on a drum, which was independently driven, while short intervals of time were marked on the paper by a clock. A great many experiments were made with this apparatus, which was found to work well, and in order to compare the efficiency of the guide-blade models with simple screws under as nearly as possible similar conditions, models of screws were made of larger diameter than their rivals, in a proportion that would use about the same power when working at their best speed. The results obtained will be found in the Table No. 1.

TABLE I.

Dimensions of Model Propellers, and Results obtained.

NAME OF PROPELLER	No. 2	No. 11	No. 3	No. 3_1	No. 10	No. 10_2	No. 10_3	No. 10_4	No. 10_5	No. 10	No. 6_4	No. 6_6	No. 7_1	No. 9_0	No. 9_2	No. 6_9
Date of experiment	July'79	July'80	July'79	...	Jne.'80	July'80	July'80	Aug'80	Aug'80	Aug'80	Sep.'79	Sep.'79	Nov'79	Apl.'80	Apl.'80	Jne.'80
Type of propeller	Common.		Thornycroft First Patent.								Thornycroft Second Patent.					
Diameter of ,,	7¾″	11·32″	9″	9″	11·32″	11·32″	11·32″	11·32″	11·32″	11·32″	8″	8	8⅞″	8″	8″	8″
Pitch on for$^{d.}$ edge	10½″	14″	9·42″	9·42″	11″	12″	11″	11″	11″	11″	10″	10″	17·75″	12·23″	12·23″	10″
,, after ,,	10½″	14″	11·14″	11·14″	13·64″	15″	14½″	13·64″	13·64″	13·64″	19½″	19½″	21·25″	71·6″	71·6″	19½″
Number of blades	3	3	3	3	3	3	3	3	3	3	2	3	3	2	4	2
Length of ,,	1½″	...	1″	1″	2½″	2½″	2½″	2½″	2½″	2½″	3½″	3½″	2½″	5″	5″	3½″
Obliquity of guides	...	...	...	...	...	...	...	...	...	...	½″	½″	¼″	7/16″	7/16″	⅜″
Number of ,,	...	...	...	...	...	...	...	...	...	...	6	6	6	6	6	6
Length of ,,	...	...	...	...	...	...	...	...	...	...	2″	2″	1¾″	1¾″	1¾″	1 5/16″
,, of casing..	...	...	...	...	...	...	...	...	...	...	4¾″	4¾″	3¼″	6¾″	6¾″	6¼″
Diameter of boss..	2¼″	2¼″	2¼″	2¼″	2¼″	2¼″	2¼″	4½″	3″	2¼″	4⅛″	4⅛″	4⅛″	4⅛″	4⅛″	4⅛″
Length of body from max. diam.	...	...	...	...	...	...	...	7″	3¼″	...	12¾″	12¾″	12¾″	12¾″	12¾″	8″
Velocity due to pitch of leading edge, in knots	6·04	5·17	4·65	...	4·62	4·7	4·43	5·2	4·75	4·6	4·1	3·62	5·83	3·52	3·77	3·7
Thrust due to 12″ diameter ...	12·6	12·1	12	...	16·3	23·4	16·8	22·5	19·1	16·3	31·5	26·4	21	32·6	50·6	22·5
Revolutions at max. efficiency	700	450	600	1,100	510	475	490	575	525	510	500	440	400	350	375	450
Thrust at ditto ...	5¼ lbs.	10¾lbs.	6¾ lbs.	17¼lbs.	14½lbs.	20½lbs.	15 lbs.	20 lbs.	17 lbs.	14½lbs.	14 lbs.	11¾lbs.	11½lbs.	14½lbs.	22½lbs.	10 lbs.
Max. efficiency ...	·659	·616	·705	·49	·635	·63	·614	·56	·63	·64	·665	·663	·645	·577	·53	·644

In this table the particulars of some of the propellers tried are arranged for each propeller in vertical columns, and headed by a number by which each propeller will be known. Numbers 2 and 11 are described as common propellers, and are of uniform pitch throughout their propelling surface, having an oval-shaped blade as in common use, and these were made for comparison with the other propellers tried, as a sort of standard by which the merits of the other propellers could be measured.

Numbers 3 and 10 are screw propellers, with the blades thrown back, and the radial centre-line of the blade is convex on the driving face. These are described on the diagram table as Thornycroft No. 1. Nos. 6, 7, and 9 are guide-blade propellers, having blades and guides much like those proposed by the Hon. Richard Parsons, but having also another feature which is important to ensure success. This consists in a large boss, which gradually contracts the area of the stream flowing through the propeller, and is followed by a body which gradually allows the accelerated stream to unite.

This table only contains the results of a portion of the models tried, but they have been selected as being those of the greatest interest and giving the highest efficiency. The first column in the table will, I hope, explain itself for the most part, but in it there are some terms used which require explanation. I believe Mr. Froude was the first to show that there was a particular speed of running for any screw propeller which gave the best results, and that this speed corresponded to that which gave about 20 per cent. slip. This speed of turning is recorded on the table in the line marked "revolutions at maximum efficiency," and is calculated for a speed of $4\frac{1}{2}$ knots, at which speed the launch was run in the experiments given.

The line "thrust at maximum efficiency" is the thrust of the propeller available for propulsion at the before-named rate of turning and speed through the water. But the figures in this line refer to propellers of various diameters, and cannot therefore directly give any relative idea of the thrust of the several propellers. Another line has therefore been prepared, marked "thrust due to 12 inches diameter," and gives the relative performance when the diameters are equal.

The line marked "velocity due to pitch of leading edge" is of interest, as it shows how far the idea has been realised of making the leading edge of the propeller cut the water without striking it.

In comparing the three types of propellers experimented on, it will be seen that the efficiency does not vary much between them, the lowest being the simple screw with uniform pitch. The experiments, however, do not show clearly which propeller should take the highest place in efficiency, for although No. 3 propeller gives ·705, No. 10 of the same type gives only ·635 on one hand, and ·64 when tried again; while Nos. 6_4 6_5 and 6_9 give respectively ·665, ·663, and ·644.

The propeller No. 3 had long narrow blades, which were inconvenient, and did not

allow the engines to be run as fast as has since been found necessary to fully utilise the capacity of the engines in the first-class torpedo boats, which are now fitted with propellers having wider and shorter blades, giving greater speed for the same boat and engines.

No. 10 is a model of the propeller now used in the second-class torpedo boats built by my firm, and was tried against a model of the common screw of the same size, No. 11 in the table; the result being considerably in favour of No. 10, which gave an efficiency of over ·635.

Propellers similar in form to these models were tried on the second-class boat No. 104, and the results obtained with the boat verified the model experiments on these two propellers.

In the comparison of efficiency, if we may exclude No. 3 and take only those propellers that may be run at a high turning velocity, the guide-blade propellers are the best, and Nos. 6_4 6_5 give above ·66. These propellers require to run too fast for the engines if fitted to the first-class torpedo boats, and the *Lightning* was fitted with one of the form of which 6_9 is a model, the efficiency in this case being about ·64.

Having found that the guide-blade propeller, fitted with large boss to contract the stream, gave such good results, I made some experiments with bosses and bodies fitted to the propeller No. 10, but the results obtained were unsatisfactory, and I was obliged to abandon the hope that the high duty obtained from the guide-blade propeller shown could be obtained by the much more simple means of fitting a suitable body behind a large boss.

The experiments on No. 3_1 in table indicate the same propeller as No. 3, but the trim of the experimental launch was altered so as to allow the ends of the blades to just break the surface of the water, as described by Professor Reynolds in one of his papers read at this Institution. The result in the experiment I made is, I now find, worse than I had imagined, for although, as I have previously stated at the Institution of Civil Engineers, there is a point reached at a very high number of revolutions, when the efficiency reaches about ·49, the thrust is then very high, and to obtain this result in an actual ship and propeller would be far beyond the capacity of the engines of the vessel.

If this propeller is used to do the amount of work it can do best, when properly immersed (namely, exert a thrust of $6\frac{3}{4}$ lbs. at $4\frac{1}{2}$ knots), when breaking the surface of the water and giving the same thrust, it will require to run at 910 revolutions instead of 600, and the efficiency will be only ·36, or about half its proper value for the propeller. It is of course uncertain what effect increase of size has on the amount of this loss, which it probably diminishes, but great improvement is required, or the loss of efficiency will still be very serious, even in large propellers.

In examining the velocity due to the leading edge of the various propellers, No. 10 has a slight excess over the undisturbed stream, which stream is equal to $4\frac{1}{2}$ knots, and in No. 10_3, in which the form is changed so as to give a greater increase of pitch in the

width of the blade, the speed of the forward edge is less than $4\frac{1}{2}$ knots, but the efficiency is reduced. This may perhaps be explained by the action a screw has on the water which is immediately in front of its disc; the water is here accelerated by the loss of pressure the blades cause, and arrives at the propeller with almost as high a velocity as it leaves it. In my guide-blade propeller this cannot be the case. There must be an increase of velocity within the instrument due to the amount of contraction given by the boss.

In the guide-blade propeller the speed of forward edge is greatly less than the initial speed of the stream in which they work, except in one of large diameter (No. 7) designed for a lower thrust, the contraction of the stream being less as the same size of boss was placed in a larger tube. As, however, lessening the contraction did not appear to bring any improvement, the opposite course was tried by again using the same size of boss, but in a smaller tube. This also resulted in a reduction of efficiency, although, as might be expected, a very high thrust was obtained.

It would therefore appear that the amount of contraction obtained in the models 6 and 9 is that best suited to propellers where a high efficiency is required, which cannot be sacrificed to give increased thrust.

I am not prepared to say the figures given represent the exact efficiency of the models tried, but I do believe they may be trusted as to the relative value.

The apparatus used was frequently tested, but the amount of friction in the propeller shaft was always considerable, and unfortunately variable in amount to some extent, and thus liable to lead to error. The launch no doubt disturbed the water slightly, but this would affect all the models to about the same extent.

I have now to ask your attention to the Table No. 2, which gives some particulars of the *Lightning*. The first column relates to the vessel when fitted with No. 3 propeller, and the other columns to the same vessel as afterwards fitted with No. 6_9 propeller, No. 6_5 in the second column having three blades instead of two.

It will be observed that the co-efficient obtained with the new propeller is good except in the last experiment, and I am informed that the Admiralty intend repeating this experiment, as there appeared to be some considerable falling-off in speed during this trial.

With regard to steering, the improvement is very marked, and the power of turning the boat against any extra resistance, which the trials do not show, is even more marked.

I would call attention also to the great reduction in the diameter of the propeller used, 5 ft. 10 in. reduced to 3 ft.

There is some doubt as to the original propeller being fully immersed even at full

speed. This makes the comparison possibly unfair for the No. 3 propeller, but there was no appearance of racing except when the rudder was put over to a considerable angle.

The arrangement of the propeller as fitted to the *Lightning* is shown in Figs. 1, 2, 3, (Plate IIA.), where the propeller is enclosed in a tube which carries the guide blades within its after-end, and the part which has been described as the body is carried by the rudder, of which it forms a part. In order to ensure ample steering power, the tube was fitted with two curved pieces fitting against the outside on either side, and these were so actuated as to come out and form an additional rudder when the helm was put over to any considerable angle, remaining in their places, however, for small angles of the tiller.

As may be seen from the drawing, this involved complicated mechanism, which in itself was undesirable; and, further, the increased thickness of the tube necessary for this construction increased the resistance of the propelling apparatus, and thereby damaged its efficiency.

Plate IIB. represents a shallow river steamer, propelled by two guide-blade propellers, the hull being specially formed to adapt the propellers to a very shallow draught. A hull of such dimensions has a very large immersed surface for the displacement, being in this case 61·3 square feet per ton, and very unfavourable to the displacement co-efficient. When this fact is allowed for, I think we must consider that this boat gives a good result, but the most curious thing connected with this shallow steamer is this: when one engine and propeller only are used, and the other propeller is still on the boat, and not turning, the performance of the vessel with one propeller appears to be better than when both are used; at the same time, their very small diameter must be remarked, and also the moderate speed of engines required. But in order to attain this last result, it was necessary to use a form of blades with exceedingly long pitch (see Table I. propeller No. 9.); this entailed reduced efficiency only ·577, and rendered the action of the propeller very imperfect when going astern, but the way of the boat could be stopped in about two lengths.

The mean pitch in a guide-blade propeller does not give a correct idea of the acceleration to be expected, and the same is true of any screw with a great increase of pitch. It is necessary, in order to compute the action of one portion of the surface, to consider the effect that has been previously produced by the surface that has already acted on a particle of the water, and to illustrate this Diagram 3 (see page 50) has been prepared.

I consider that there are two kinds of vessel where the guide-blade propeller might be used with advantage: they are sea-going vessels which often run in ballast, and, consequently, in their light trim do not properly immerse a common propeller, and the other vessels for navigation where the draught of water is limited and necessitates the use of paddles or guide-blades. In this case it would appear that the guide-blade propeller may be more advantageously used than the paddle where high speed is required, owing to

the reduction in the weight of the machinery, consequent on the increased number of revolutions that may be used, at the same time retaining the light draught due to the paddle-wheel, and having the further advantage of not increasing the extreme width of the vessel, which is an important feature in inland navigation.

In conclusion, I have the pleasure to acknowledge the assistance given me by Mr. Sidney Barnaby in making the experiments, which form the most important part of this paper, and to thank my other assistants for the great pains and care with which they worked out the results.

TABLE II.

H.M. Steam Torpedo Vessel "Lightning."

	STOKES BAY.	THAMES.		STOKES BAY.
	May 22, 1877, No. 3 Propeller.	No. 6_5 Propeller. April 26, 1881.	No. 6_9 Propeller. June 2, 1881.	Jan. 1883, No. 6_9 Propeller.
Displacement	28 tons	34 tons about	34 tons about	34 tons
Indicated H. P.	400·8	384	477	428
$\frac{V^3 D^{\frac{2}{3}}}{I.H.P.}$	147	151	151	113
Revolutions of Engines per minute	354	390	423	
Speed in Knots	18.54	17·7	19·02	16·65
Time required to make complete circle	S 3–50, P 3–50 Full Power. S 3–13, P 3–48 Half Power.	...	...	S 1–53, P 1–34 S 1–3, P 1–3
Diameter of circle in yards ...	155, 155 Half Power.	...	...	104, 92 Full Power 94, 94 Half Power
Diameter of Propeller	5ft. 10in.	3ft.	3ft.	3ft.
Number of Blades	3	3	2	2
Immersed surface per ton displacement at 34 tons displacement ...	...	24·1	24·1	24·1

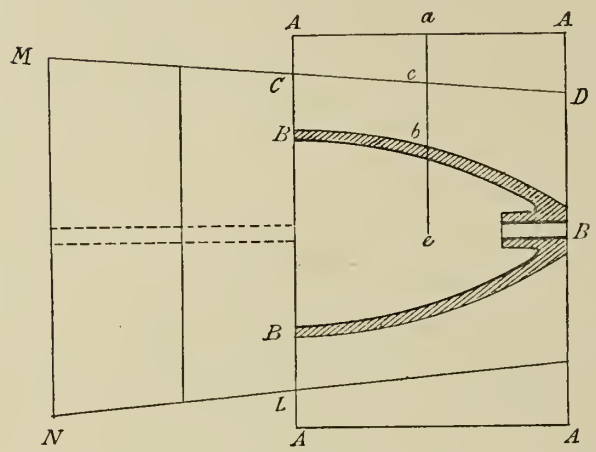
Table III.

Shallow Draught River Steamer.

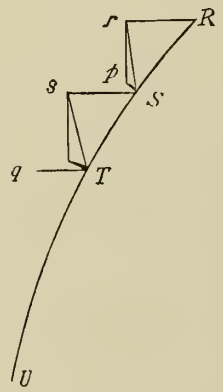
Date	October, 1882.	18 October, 1882.
Displacement	9·23 tons	9·2
Slip	44·1 p.c.	...
$\frac{V^3 D^{\frac{2}{3}}}{I.H.P}$	85·2	112
Revolutions per minute	480	...
Speed in knots	10·49	8·8
Diameter of Propeller, No. 9_0 ...	16 in.	16 in.
Number of Blades	2	2
Immersed surface per ton displacement at 9·8 tons displacement	61·3 sq. ft.	61·3 sq. ft.

APPENDIX.

Note to explain Diagram No. 3.



A A A A is a section of the propeller, B B B is the boss. The mean path of the water which flows through the screw is along a surface of which C D is a section, any point c of which is determined by the equation $ae - ce^2 = ce^2 - be^2$, so that C D divides the volume swept out by the blades into two equal parts. The line C D is nearly straight, so let it be supposed to be straight. Then, to get a section of the blade along the mean path, a cone of wood C M N L is made such that its surface is a continuation of C D. Then, having fitted the cone and screw on the same axis, a sheet of paper is laid round the cone and is cut so as to fit the blade along C D.



Let R U be the section of the blade thus obtained. Draw Rr perpendicular to the direction of the shaft, and equal to the velocity of the point R round the shaft, and draw $\underline{rp}$ = the velocity of the boat or of the water relatively to the boat. Then, while the blade travels from r to R, a particle of water will travel from r to p with the motion due to the original velocity, but as the particle must still be in front of the blade, the blade will have pushed it through a distance = p S in a direction

perpendicular to R S. Thus, $\overline{r\,S}$ will be the velocity and direction of the particle at S. At S set off S s equal to the velocity of the blade at S, and draw $\overline{s\,q}$ equal and parallel to $\overline{r\,S}$, and repeat the process as before till the end of the blade is reached. Let V be the velocity of the particle at the end of the blade; this will be the velocity relatively to the boat, and the slip will be $V - \overline{r\,p}$.

DISCUSSION.

Mr. W. H. White: I am sure I am expressing the sentiments of all members of this Institution who have made any study of steam propulsion, when I say that we owe to Mr. Thornycroft the deepest debt of gratitude for giving us these results. To anyone who has had to do with experiments at all, it must be obvious, as I have the pleasure of knowing by the personal facilities which Mr. Thornycroft has given me on various occasions, that there is an enormous amount of work and most patient research represented here in a form most convenient for other people to use, and Mr. Thornycroft has here put upon record information which I am sure will have an important bearing on the future of steam navigation at high speeds. That is my conviction. Having said that, may I ask Mr. Thornycroft if he will be good enough to give one or two other pieces of information which I think he has, but which I cannot find in the paper. I do not find in the paper—I may be wrong; I have only glanced through it casually—any clear statement of what increase in effective thrust with a given propeller Mr. Thornycroft considers to be gained by the use of the guide-blade and casing. I believe Mr. Thornycroft has that information, and it will be a most valuable addition, if I am right in thinking it is not here. Then I should like Mr. Thornycroft, if he can, to give us what the strength of the attachment for the guide-blade and its casings would need to be to prevent anything in the nature of rotary motion, if this system were applied on a large scale. It seems to me, if I apprehend the matter rightly, that so far as the guide-blades help the effect there would be that tendency, and it would have to be guarded against; that is, in getting the utilisation of the longitudinal component of the thrust you have to deal with another component, and that would mean, I think, some considerable strengthening of the casings and their attachments with large engine-power. Next, would Mr. Thornycroft add the slip curve to this diagram?—it would be such a complete comparison then with the late Mr. Froude's screw experiments. Mr. Froude gave curves of that kind, if I recollect rightly, connecting the efficiency with the speed of revolution and slip. Then with regard to the suggestion as to the possible performance of that shallow-draught steamer being better with a single screw than with two screws.

Mr. J. D'A. Samuda: Only in turning.

Mr. W. H. White: In propelling. If I correctly gather the facts in Table III., when she had the two screws at work she was certainly being driven at a speed which was considerable in relation to her length. Is it not possible that with a vessel of that form when driven at speeds above eight knots, you are getting beyond the limit at which the curve of power expressed in terms of speed-abscissæ would run up very rapidly? I don't know whether Mr. Thornycroft has that information, but I simply make the suggestion, and again thank him for his very valuable paper.

Mr. N. Barnaby: I was hoping, my Lord, that some marine engineer might favour us with some observation, but as none has risen I shall be glad to be allowed to say how strongly I feel with Mr. White the benefit of having information of this kind placed so freely before us as Mr. Thornycroft

has been good enough to place it. We have all of us heard of the things he has done with the *Lightning* and with that small vessel for the Congo, but we are not accustomed to having such early information given by people in his position enabling everybody to benefit by the experience he has gained by his careful calculations and experiments.

Mr. Frank Marshall: My Lord, as a marine engineer I am sure I only express the indebtedness that the whole of my profession owe to Mr. Thornycroft for this very valuable contribution to the proceedings of this Institution. It is only one of the many debts of obligation which Mr. Thornycroft has laid us under in the various improvements that have come under our notice bearing on our profession. There is one difficulty I think we all labour under, if I might suggest it here, in connection with these papers, and that is the difficulty of our not having them in our hands before the meetings. I notice in the report of the Council that it is the intention to publish these papers and to distribute them immediately after the sittings. Perhaps the Council will take into their consideration the great utility it would be if we had the papers put into our hands before the meeting, in order that such subjects as Mr. Thornycroft has brought before us to-day might receive the attention that they so richly merit. It is quite impossible, coming to a meeting of this sort and hearing for the first time such problems enunciated as Mr. Thornycroft has brought before us to-day, to discuss them with that advantage which we might otherwise do. I strongly urge this upon the Council. I again pay my tribute of thanks, and I am quite sure I only speak for my whole profession when I say that Mr. Thornycroft has in this, as in many other instances, added very valuable information to our stock.

Mr. J. I. Thornycroft: My Lord, and gentlemen, I have much pleasure in answering as far as I can the questions that Mr. White has put to me. I think the Table I. (p. 43) I have put on the wall will assist him in some part. Mr. White asks what increase of thrust we could get, as I understand him, beyond what we are getting with the propeller now.

Mr. W. H. White: Yes.

Mr. Thornycroft: In the Table there is this line, "Thrust due to 12 inches diameter." That line makes all the propellers equal in diameter, and gives them working at their best efficiency; so that by comparing what you find in that line you can see how much more you can tax one of those propellers than you can tax an ordinary propeller and still get the best work. It is the fourth line from the bottom. There you will find that with an ordinary propeller of 12 inches diameter at $4\frac{1}{2}$ knots you can have a thrust of about $12\frac{1}{2}$ lbs. Then in examining the guide-blade propellers you will find on the same line the first one is propeller No. 6_4. That propeller for its best work gives a thrust of 31 lbs. instead of $12\frac{1}{2}$ lbs., at the same time the efficiency being about equal. There you have about two and a half times the thrust for the same diameter. What I had settled in my mind previously to writing this paper was that I could get twice the thrust for the same diameter, and that curiously enough (experiments seemed to prove that at first) amounts to this, that with an ordinary propeller you lose a certain amount (by an action similar to walking up more stairs than you get up), and also you lose a certain amount by friction. If you could have your stairs, instead of slipping down in that way, standing still at first, and only slipping towards the last, you could afford the stairs to slip away from you twice as fast and lose only just as much. That would lead you on theoretical grounds to expect that you could use just twice the thrust, and in designing the propeller for the *Lightning* we made that assumption—that we could use twice the thrust. In designing this shallow boat for the Congo it was very desirable to use smaller propellers

than these, and in examining curves, one of which we have here, which represents a 9 in. propeller, there you see you can work to 15 lbs. and get the best result. This is an 8 in. propeller. You can work up to nearly 30 lbs. from that 8 in. propeller without any very great loss. Of course that may be said of a common propeller. You can work it without great loss beyond its best work, but in designing a vessel we must use our propeller to do its best work. Again referring to this boat, there we had to use a very small propeller, and we had to get a very large thrust. We made the propeller with a very great increase of pitch and a very great contraction, but we found with a less contraction for obtaining a long pitch we got a very high thrust. Propeller No. 90 with two blades when reduced to 12 in. diameter gave a thrust for its best work of 32 lbs. Of course it is not certain that the propeller was put exactly to the amount of work it was intended for. The real facts of the case turned out to be these: when the boat was actually built it was found that one propeller would drag the boat and give a higher displacement co-efficient than it did with both propellers in use. I am not prepared to explain that.

Mr. W. H. White: I mean, if I may explain, that in estimating the displacement co-efficient, it is assumed that the resistance is varying as the square of the velocity, whereas, as a matter of fact, it was probably varying from 9 knots to a higher speed in a greater ratio than that.

Mr. Thornycroft: I am afraid I am dwelling too much on one of Mr. White's first questions. I think possibly Mr. White is right in what he said about the lower speed of the boat, and that 12 knots an hour was beyond its best speed. That was also borne out by the fact, as will be seen in Table III., that at about 8 or 9 knots the displacement co-efficient is very much higher than at lower speed. And had the boat been intended for 8 or 9 knots instead of 12 miles the one propeller would have been the best, but at 10 knots the one propeller still seemed to be the best. With regard to the attachment of the guide-blades to the vessel, it seems evident that if the guide-blades do what they are intended to do, if they take nearly all the rotary motion out of the water, again, you will have exactly the same turning movement on your guide-blades as you have on your shaft, and the attachment will want to be just as strong as the fixings of the engine in the boat. Mr. White has asked also for something I am afraid I really cannot give him. He wants the slip curve. There is one difficulty with this propeller in experimenting. The propeller was not all fast to the shaft, and this could not be attached without friction to some extent. In order to experiment with it, it was necessary to run it at such a pace that tube and guide-blades would propel itself. We have worked down as low as they would keep forward; when these would no longer keep forward we knew we were doing bad work, and we did not go below that, although it was quite easy to do so with a simple screw as Mr. Froude had so well done, and really the method of that diagram is taken from Mr. Froude's. We were not able to continue this first line as Mr. Froude did down below there. There was also another reason why we did not do that. We found we were working our experiments at about what we thought was the best speed—we could not tell when we were doing that which was the best; it took us a day or two to get the results out —we had enough work to do to try the experiments where we knew there was something to be found, and not to be working on theoretical considerations. I must thank you very much for kindly hearing me at such length.

The President: Gentlemen, I am sure you will allow me to thank Mr. Thornycroft cordially on your behalf for his valuable paper. I should like to take this opportunity of making a few remarks upon a matter which has been brought before the meeting, because when you want a thing done there is nothing like hammering at it until you get it done. I have always

found that in life. My friend Mr. Marshall has pressed a point which I certainly should have pressed if he had not raised it, because it is one of considerable importance to our members, that is, the getting of these very theoretical and difficult papers, raising extremely difficult problems, into the hands of members sooner. I am quite sure that our discussions would benefit very largely if that could be done. I have pressed it in former years, and I again press it now, and I am sure you will all agree with me on the extreme importance of having before the meetings papers in which problems of this difficulty are raised ; and that they are difficult you can see from the fact that a man of the extreme ingenuity and ability of Mr. Thornycroft hesitates a little in answering the questions put by another extremely able man. That shows in itself that the ablest men are unable always to answer criticisms upon very difficult problems off-hand, before they have been able to work the thing out either by experiment or otherwise. I think whenever these papers are ready, and I would at once press this on our members, with great respect to them, that it would be for the benefit of our discussions, particularly when they are papers raising such extremely difficult problems, that they should be placed, if possible, in the hands of the members some days before they come up for discussion. The discussion loses by it if that is not so, because gentlemen naturally feel a modesty in getting up and dealing with matters of this difficulty at an extremely short notice. I pressed that before, and I press it again. I have only now on your behalf, at the end of one of our longest sittings, I am bound to say, to thank our friend, Mr. Thornycroft, for his very admirable paper.

ON THE STEAM TRIALS OF THE "SATELLITE" AND "CONQUEROR" UNDER FORCED DRAUGHT.

By R. J. Butler, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The many advantages which are obtained in torpedo boats and other small craft fitted with a single boiler, by the successful employment of forced draught by fans in closed boiler-rooms, have naturally induced the designers of war ships and their machinery to endeavour to secure similar advantages, by the application of the same system on a larger scale to the common high-pressure boilers, which are usually fitted in such ships.

For some considerable time past provision has been made in the designs of the machinery of all the most important ships of the Navy for the application of this system, so that it only remains to order the addition of the requisite boiler-room fittings, in case of its adoption.

The French Government have used the fan draught with ordinary boilers in closed boiler-rooms in ships of war for some two or three years, and in this country the firm of Sir W. Armstrong & Co. have adopted and carried it out successfully, in connection with low circular boilers, in some fast cruisers which they have built during the last two years for foreign Governments.

Within the last twelve months, the *Satellite* and *Conqueror*, fitted with boilers of the usual types, were far enough advanced to admit of the necessary additions being made to their boiler-rooms, for putting them under air pressure, without delaying their completion and trials. The opportunities thus afforded of acquiring needed information were embraced; the arrangements have been carried out in these vessels, and several steam trials made.

The engines of the *Satellite* and *Conqueror* were not designed to develop the high powers which it was found the boilers could generate steam for, even when they were worked under a moderate amount of air pressure. The conditions, therefore, were not favourable for the determination of the best possible results, but those which were

obtained, are of sufficient value to enable the capabilities of the machinery to be more nearly proportioned to the boiler power in other vessels. It is in the hope that the particulars of the trials made in these two vessels may be of interest to the members of this Institution, that this paper has been prepared by me.

The *Satellite* is a single screw composite sloop of 1,420 tons displacement. Her engines are of the usual two cylinder compound type, placed horizontal. The boilers are of the long low kind, with two furnaces in each, an intermediate fire-box, and the tubes beyond. The boilers are arranged in pairs in two separate water-tight compartments, as shown on Figs. 1, 2, 3 (Plate III.), being fired at the forward and after ends respectively. One funnel serves both sets of boilers. The engines and boilers are placed beneath a steel deck, which is below the water line. All the necessary openings in this deck for ventilation and other purposes are protected by shutters or stout bars, and coffer-dams. Direct communication between the boiler-rooms, and to the back ends of the boilers, is obtained by means of a passage at one side. An air lock is provided at each end of this passage, and also at the entrance to the after boiler-room from the engine-room. Air-tight screens of thin sheet iron are worked flush with the fronts of the boilers, attached to the fore and aft coal-box bulkheads at the sides, and to the steel deck and ship's bottom. Doors are provided in these screens to give access to the tops of the boilers. Each boiler-room has a fan five feet in diameter, placed horizontal under the steel deck, with separate air supply shafts extending well above the upper deck. Other air shafts and openings are provided for ventilation and access to the boiler-rooms, but these are closed when the forced draught is applied.

Two sister vessels, the *Heroine* and *Hyacinth*, have machinery and boilers exactly similar to those of the *Satellite*, except that they are not fitted with the apparatus for working under air pressure. The particulars of their steam trials are given at the end of the paper, because they form an interesting comparison with those of the *Satellite*.

The *Conqueror* is an ironclad ram of 6,200 tons displacement, having twin screw engines of 4,500 estimated I.H.P. Her engines are vertical inverted three cylinder compound, with cranks at equal angles. A middle line bulkhead separates the two engine-rooms. There are eight high boilers with return tubes over the furnaces. They are arranged in pairs in four boiler-rooms, separated from each other and from the engine-rooms, by water-tight bulkheads. The boilers are placed with their backs to the middle line bulkhead, and are fired from the wings. One funnel is common to all the boilers.

The only additions made to the original arrangements in the boiler-rooms consist of a horizontal ceiling (Figs. 4, 5, Plate III.), about eleven feet above the floor, worked across each room from the coal-box bulkheads to the fronts of the boilers enclosing the uptakes; and vertical screen plates between the boilers extend up from the front boiler bearers to meet the ceiling. The vertical plates are also worked round the fronts of the boilers

enclosing the smoke-boxes, which thus keep the boiler-rooms cool. These vertical plates are far enough back from the fronts of the boilers to form pockets for the gauge glasses.

Hinged doors are fitted along the ceiling to be open under normal conditions of working, so that the original system of ventilation by means of cowl pipes from above the upper deck then operates. Air locks are provided between the engine-rooms and after boiler-rooms, and also the passage-way across the ship at the forward end of the other boiler-rooms. When working with all the boilers under air pressure, communication is open between the forward and after boiler-rooms through doorways in the cross bulkheads.

Two vertical 4-feet fans on one spindle, fixed at the after ends of the rooms above the ceiling, supply each of the after sets of boilers; and one 5-feet fan at the forward ends of the rooms supplies each of the forward sets of boilers.

The air supply for the after fans is obtained entirely from above the upper deck through separate vertical shafts. The forward fans are supplied partly from the main deck and partly from the upper deck. The principal particulars of the machinery of the *Satellite* and *Conqueror*, as well as the most important of the observations made on their trials, are also given at the end of the paper.

The *Satellite* and her two sisters were all tried under natural draught alone. Trials were also made in the *Heroine* and *Hyacinth*, using steam jets in the funnels to compare the effect with that of the fan draught. In the former vessel, only the forward boilers were worked in this way, but in the *Hyacinth* all the boilers were used, and they were worked as hard as possible in both cases.

The I.H.P.'s developed on these two series of trials agree with each other very fairly, considering that they were made under somewhat different circumstances. The intended horse-power of these vessels was 950, to be obtained without any forcing, but the result considerably exceeded this in each case, the mean power of the three, under natural draught simply, being about 20 per cent. greater than the contract power. This was owing, no doubt, to the exceptionally good ventilation, the high funnel, and to the high steam pressure used. The performance was very satisfactory, but it had the effect of narrowing the scope of the working under forced draught in the *Satellite*.

These trials show that under the most favourable conditions, from 10 to 10½ horse-power is obtainable per square foot of grate from this class of boiler, when worked without forcing the draught; and that nearly 13 horse-power, or about 24 per cent. more, can be realised when the ordinary steam blast is employed, the boiler-rooms being open as usual. It is to be observed that the power obtained by forcing the draught in the boilers of these vessels by steam jets, compared with the results from other similarly proportioned boilers, is also exceptionally high, which is only attributable to the favourable conditions above

referred to. The proportionate *increase* of power produced by these means, over that given by the natural draught alone, is, however, not exceptional.

On the *Conqueror's* first trial, scarcely 8 horse-power per square foot of grate was obtained. But here the conditions were not so favourable for the development of a high result as in the smaller ships. The grate bars were unusually long, and the closed-in boiler-rooms rendered a very slight use of the steam blast necessary at times to quicken the draught. As the effect obtained from these boilers on this occasion was less than those which have been previously obtained under natural draught alone, from the same type of boilers similarly arranged, this can only be regarded as equivalent to a natural draught trial. The load on the boilers was 20 lbs. less than on those of the *Satellite*, but the boiler tubes are brass (ferruled at the fire-box end), whereas those of the *Satellite* are iron.

The performance of these boilers is, however, not really so bad as it appears to be, judging it with reference to the grate area. In comparing the effect obtained from exactly similar boilers, the grate area forms a safe basis of comparison; but in dealing with different types of boilers, such as those of the *Satellite* and *Conqueror*, this does not necessarily hold, seeing that in one case, as in that of the high boilers of the latter, the proportion of grate area may be abnormally increased, by extending the grate bars to the backs of the fire-boxes. The maximum grate area obtainable was employed in both types on these trials, that of the *Satellite* bearing nearly the usual ratio to the total heating surface, which is fixed, while that of the *Conqueror* was greatly in excess. The proportion of heating surface provided in both types of boiler to the estimated horse-power is practically the same.

A comparison of the power obtained per square foot of grate on the *Conqueror's* first trial with the mean result of the natural draught trials in the other vessels, makes the performance of the boilers of the latter appear about 30 per cent. superior to that of the high boilers; but, comparing the mean power realised, in proportion to the total heating surface, as it properly should be compared, the apparent superiority of the low boilers becomes reduced to 9·5 per cent., and, as above mentioned, the favourable circumstances which obtained in the small vessels, and the unfavourable ones in the *Conqueror*, account for this.

The first of the *Satellite's* forced draught trials, like the first steam blast trial, was made with the two forward boilers only, to keep well within the limits of the capability of the engines to transmit the power. The air pressure used varied from 1 to 1½ inches of water, but was in effect only an inch throughout, the increase being required gradually as the heating surface became diminished by the lower tubes being blocked up by coal thrown over the bridges. 864 I.H.P. was realised as the mean of the three hours' work, or 15·7 horses per square foot of grate, and this power was maintained during the whole time as uniformly as could be expected. The tendency of the boilers to prime prevented this air pressure being exceeded.

On the next trial all the boilers were used, and worked for half an hour at $\frac{1}{4}$ inch air pressure. The results of this are only given as showing that this amount of air pressure with the boiler-rooms closed, produced practically the same effect, in regard to power, as the natural draught with the boiler-rooms open.

On the subsequent trial three boilers were used, the air pressure being increased gradually. With $\frac{1}{2}$ inch, the effect is very nearly the same as when using the steam blast.

At an air pressure of 1 inch, 16 horse-power was obtained per foot of bar, but it will be noticed that the condensing apparatus, which maintained a good vacuum up to about 1,200 horse-power, was being overtaxed, and a fall in the vacuum took place.

The three boilers were further worked at an air pressure of from $1\frac{1}{4}$ to 2 inches. With the increase in air pressure, the vacuum was further diminished, but the power and speed of the engines were maintained with a very fair degree of uniformity. Apparent unsteadiness in the water caused the feed to fluctuate, and necessitated variation in the air pressure to keep the power uniform. The I.H.P. now obtained reached as high as 1397, or 16·9 per foot of grate bar, as the mean of the two hours' trial. This exceeds the performance under natural draught by $62\frac{1}{2}$ per cent., and that under steam blast by 30 per cent. Considerable leakage of air occurred through the furnaces of the unused boiler; especially at the higher air pressures, so that an increased fan speed was necessary in one boiler-room to maintain the required air pressure.

After this, an attempt was made to work all the boilers, commencing with $1\frac{1}{2}$ inches of air pressure, but the engines could not take the steam, and it was abandoned. The indicator diagrams taken showed that 1,570 horse-power had been developed, or about 65 per cent. above the specified power of 950 horses, the engines running at 126 revolutions.

The indicator diagrams were taken, and the usual observations made every quarter of an hour, during the forced draught trials in this ship.

In the *Conqueror* two series of forced draught trials took place. The first, which immediately followed the ordinary trial, was made solely with the object of ascertaining the maximum power and speed the vessel could attain on an emergency, and it is only valuable on that account. Steam blew off freely from the safety-valves during the whole time, and additional pipes were fitted to admit steam direct from the main pipes to the low pressure cylinders. Thus a large quantity of the steam generated was wasted, and another large portion used uneconomically.

The other trials were made to determine the capabilities of the boilers, and for this purpose the after boilers only were used. They were worked for $1\frac{1}{2}$ hours, under an air pressure varying from 1 to $1\frac{1}{2}$ inches in the two rooms, and subsequently for the same period at from $1\frac{1}{2}$ to 2 inches of air pressure. The I.H.P.'s developed under these conditions were 3665 and 4023, or at the rate of about 12·2 and 13·4 horses per square foot of

grate respectively. The same length of grate bar, viz., 7 feet 6 inches, was used on all these occasions, and there is no doubt that the high boilers were worked at a considerable disadvantage on this account, as compared with the low boilers of the *Satellite*. But as one object of the trials was to ascertain the workability or otherwise of the long bars, they were retained. Unfortunately, circumstances did not permit of further trials being made with the bars shortened, so as to reduce the grate area to the usual proportion, for they would have yielded useful information.

If the performance of these boilers on the last trial, which was the best that, I believe, could be maintained under the circumstances, be compared, on the basis of the proportion of power obtained to total heating surface, with that of the *Satellite's* boilers when the greatest effect was realised, it will be seen that the latter still appear superior in steaming power, and to about the same extent as before, viz., 9·6 per cent., although the mean air pressure was slightly less. Comparing, again, the first trials of the *Satellite* and *Conqueror* under forced draught, and regarding the air pressures as practically the same, the performance of the boilers of the former appear 10 per cent. better than that of the high boilers. But on a short trial made in the basin at Chatham, using one of the after sets of boilers only and one set of engines, the grates then being 6 feet 6 inches long and the air pressure 2 inches, 2,140 horse-power was developed, which is at the rate of 16·46 per square foot of grate, and 0·625 per square foot of heating surface. This approaches more nearly the best of the performances of the *Satellite*, which is only superior to it to the extent of 2·56 per cent., but which was obtained with considerably less mean air pressure. It must be observed that on this occasion the fires were clean, and the men fresh, but as a considerable margin of power remained in the fans, it is an open question whether this power could be maintained as long as the other trials lasted.

It is certainly a matter of great importance in war ships to make the grates as long as the furnace barrels will admit, provided they can be efficiently worked, for long grates mean less width of boilers and reduced weight; but it is doubtful whether, on the trials of the *Conqueror*, the bars were not too long for the width of the furnaces; it was very difficult to fire the backs of the long grates effectively, owing to the thick fires which had to be maintained, and I do not think it was done. The fluctuations which occurred in the air pressure pointed to the conclusion that uniform fires were not being kept, for the feed was very regular—much more so, in fact, than in the *Satellite*, where the water was always more or less unsteady.

The total weight of the *Conqueror's* boilers, with all their fittings, is less than that of the *Satellite's* in proportion to the horse-power developed under natural and also under fan draught, and both sets of boilers are average specimens of their classes. (Double-ended boilers would be lighter still.) If their weights relatively to the power obtainable by using the steam blast could be compared, then probably the *Conqueror's* boilers would, as usual, appear heavier than the *Satellite's*, for the steam blast is not nearly so effective

in producing increased power in the return tube boilers as in the low ones. The risk incurred in working high boilers under air pressure is undoubtedly less than in working low ones, but they require harder forcing, apparently, to produce the same effect; the difference in their steaming power, however, could easily be made up by using larger fans to obtain a little additional air pressure; further, seeing that the high boilers have also a slight advantage as regards weight, it would appear preferable to employ them in connection with the fan draught, rather than the low type, in vessels of war where the height and protection will admit.

The fan speeds necessary to maintain the different air pressures were noted with the 4-feet fans of the *Conqueror*, and with the 5-feet fans of the *Satellite*, whilst the boilers were worked. Those of the *Satellite*, at 400 revolutions per minute, sustained 1½ inches of air pressure, and each addition of 50 revolutions produced approximately an increase of 0·3 inch. In the *Conqueror*, 600 revolutions gave an inch with the long grates, and 1·2 inches with the 6 feet 6 inches grates, and additions of 100 revolutions increased the air pressure to the extent of about 0·5 inch each.

The number of revolutions required for the *Conqueror's* 4-feet fans at full power would probably be too great if they had to be worked frequently and for long periods. In other vessels intended to be worked in this way a fan will be placed at each end of each boiler-room, or near the centre, as in the *Satellite*, to equalise as much as possible the draught in all the furnaces. The fans are also to be capable of maintaining an air pressure equal to 3 inches of water at full power, which will ensure a moderate rate of speed for the fans and their engines at all times. In the *Conqueror*, want of equality in the draught to the fires, owing to the fans being at one end of the boiler-room, was very noticeable, but in this case a better arrangement could not be made, nor could larger fans be employed.

In feeding the low boilers care was required to prevent the water exceeding 2 inches in the gauge glasses, or about 6½ inches over the crowns of the fire-boxes. Whenever it rose above this height, slight priming immediately set up. This, however, was probably more apparent than real, for it occasioned no inconvenience at the engines. The water in the high boilers, on the other hand, was perfectly steady, no symptoms of priming being seen at any time, although the water was allowed to rise occasionally as high as two-thirds of the glasses.

In all the vessels referred to, a separate small engine, instead of the usual pumps worked off the main engines, is provided for feeding the boilers, and placed in each engine-room. These draw only from the feed tanks, and supply all the boilers. Their speed is therefore regulated independently of that of the main engines, but in accordance with the wants of the boilers. Each boiler-room is fitted also with a small engine connected with the feed tanks and sea, so that the person in charge of each set of boilers has an independent command of his feed. This arrangement was found to work remarkably well, and

proved to be very convenient on the trials, especially in the *Satellite*. The feed engines having two double acting pumps, the supply of water to the boilers was very uniform, and it was practically free from air. The pressure-gauges on the delivery pipes showed a variation of not more than 5 lbs., whereas with the ordinary feed pumps a variation to the extent of the boiler pressure at each stroke is commonly observed. This, no doubt, operated in some degree to keep the water steady in the boilers. All the feed-engines as well as the fan-engines in these vessels are arranged to exhaust into the condensers, and also into the atmosphere.

The boilers were in each case filled with fresh water for the purpose of the trials. The best Welsh coal was used, and the stokers were trained men from the Reserves.

It is to be regretted that no estimate can be given of the quantity of coal burnt under this system of forcing the combustion, the trials being made almost entirely with the object of observing the behaviour of the boilers, and of realising the maximum power obtainable; and being in a certain degree progressive, they were necessarily of short duration—too short, in fact, to admit of any account being taken of the rate of combustion of the fuel which could be considered of value. One thing appears certain, viz., that as the air pressure was advanced, the increase in the consumption of fuel proceeded at a much higher rate than did that of the power given out at the engines.

This is borne out by the observations made of the temperatures produced in the uptakes. On the forced draught trial of the *Satellite* with the two forward boilers, a pyrometer fixed to the uptake registered a temperature from 1,000° to 1,200° F., whilst on the steam blast trial of the *Heroine* a pyrometer, similarly placed, recorded from 775° to 850°. Also, on the trials of the *Conqueror*, pyrometers were attached to the funnel just above the junction of the uptakes. On the first occasion, under the ordinary draught, the temperature varied from 275° to 280°, when the steam blast was off. When the blast was on, it varied from 490° to 600°. On the trial immediately following, when the fan draught was used, the temperature registered ranged from 850° to 1,000°.

The forced draught trials made in these two vessels can in reality only be regarded in the light of experiments, carried out with the general object of making the ground sure for an extended application of the system, and although they are not so complete as could be desired, they have yielded useful information. The results obtained are sufficient to show that, with engines of suitable size, the steaming power of the low boilers can, by employing forced draught, be increased by about 30 per cent. beyond the maximum power hitherto obtainable with the steam blast, and that the increase of effect is even considerably greater than this in the case of the high boilers.

The advantages which follow the successful application of this system of forcing the draught in the boilers of war ships are of considerable importance. It enables the machinery to be constructed within the limits of the space and weight which are sufficient

for their ordinary services, and admits of the reserve of power being stored in the light fans and fittings, instead of in the cumbrous boilers and machinery, as heretofore. Many of the existing ships might also, at little cost and in a very short time, have their power and speed materially increased by the addition to their boiler-rooms of the few comparatively light and simple fittings.

Neither in the generation of the steam nor in its employment in the engines is economy to be expected by this method of working the boilers, but neither is it necessary for the few and comparatively short periods that they will probably require to be forced in this manner. The adoption of the fan draught system has this advantage: it tends to facilitate economical working rather than otherwise under ordinary conditions, because it enables relatively smaller cylinders to be used.

With regard to the endurance of the boilers, there can be no doubt that the frequent use of the forced draught would produce a great diminution in the life of those parts subjected to the intense heat. But probably, under the conditions that will obtain, this will not be of serious moment. An examination of the boilers of the *Satellite* and *Conqueror* after the trials, showed that they had not suffered to an unusual extent by the exposure to the intense heat for the short time the trials lasted. Only two or three of the iron tubes were found to be weeping in the low boilers, and in the *Conqueror's* a few seams and rivets and about twenty of the tubes were leaking slightly.

The machinery of all the vessels above referred to was constructed by Messrs. Humphrys, Tennant & Co., of Deptford, and worked in an eminently satisfactory manner throughout the trials, especially so in the case of the *Satellite*, the engines of which were tested to a much higher degree than was stipulated for.

Some of the points to which I have made reference in connection with these trials, may seem to be of little general importance, but in my opinion they are all of more or less value to Engineers who have to provide for the development, by fan draught, of the extra power required in the first instance, and its reproduction on emergencies. It is a question, however, which appears to me to be well worthy of consideration, whether the employment of this system of forcing the draught in ordinary boilers, could not be extended to other vessels besides war ships.

It would probably not be found advantageous to adopt it generally in vessels which make voyages of longer duration than six hours, or which run continuously, on account of the delays which cleaning the furnaces and tubes would involve, and the additional wear and tear which would be incurred. But the classes of vessels for which it would appear to be particularly well suited are mail or passenger steamers which make short passages at high speed, occupying about six hours or less, and in which economy of fuel is of minor importance compared with that of keeping good time. There are many services of steamers around our coast in connection with the railway system, such as those which run

from Dover, Folkestone, and Newhaven to the French ports, the Dublin and Holyhead boats, and others, to which this is applicable. These boats are mostly paddle-wheel, so that very little, if any, benefit would be gained by supplying the existing vessels with the apparatus for applying air pressure, because, the speed of the engines being restricted, the additional steam generated could not be utilised.

In future vessels designed for these services, however, the piston speed could be increased relatively to that required for the wheels, and without adding to the weight of the existing machinery; the additional power obtainable by the fan draught could be made available by increasing the speeds of the ships, thus shortening the time of passage; or, the same speed of vessel could be obtained for a considerably less weight of machinery, if desired.

The merits which the fan draught system has to recommend it for such purposes are—

That the extra apparatus required is simple, light, and comparatively inexpensive. It admits of the air pressure being applied and removed readily. Fans of sufficient size to run at a moderate speed, having engines of simple construction, are not likely to give trouble, if properly attended to. The system is unquestionably effective, seeing that the productive power of ordinary high pressure boilers can be increased by its agency at least 30 per cent., and probably more. No extra skill is necessary for the manipulation of the apparatus, but a little additional care and attention would probably be required. Provided these be given, there is no reason to suppose, judging from the short experience of its working in the *Conqueror*, that any serious risk to life or property would be incurred by using the forced draught in connection with high pressure boilers of the types commonly employed in the mercantile marine. In the class of passenger or mail steamer referred to, steam could be raised by the fans to full pressure for starting, whilst the passengers are getting on board, thus the inconvenience to which they are frequently subjected, by the roaring off of the steam, until the boat gets under way, would be avoided.

These numerous points are collectively of sufficient importance to entitle such an extension of this system, in my opinion, to consideration.

NATURAL DRAUGHT TRIALS.

	Satellite.	*Heroine.*	*Hyacinth.*
Date of trial	April 3, 1882.	May 30, 1882.	July 25, 1882.
Duration of trial ... ... hours	4	6	3
Number of boilers used	4	4	4
Mean steam pressure in boilers ... ... lbs.	84	82·6	81·6
,, vacuum ... ... inches	26·5	26·1	26·0
,, number of revolutions per minute... ...	98·52	104·2	105·8
,, pressure—lbs. { high	32·36	32·07	31·6
,, pressure—lbs. { low	13·85	12·9	14·1
I.H.P. { high	493 } 1,116	515 } 1,127	515 } 1,195
I.H.P. { low	623 }	612 }	680 }
Grate area ... ... square feet	110	110	110
I.H.P. per square foot of grate	10·15	10·25	10·87
Tube surface of boilers per I.H.P. square feet	2·18	2·16	2·03
Total heating surface, per I.H.P.... square feet	2·61	2·59	2·44
Wind-force	4	1 to 4	2 to 4
Sea	Smooth.	Smooth.	Moderate.
Diameter of screw ... ... feet	13′ 0″	13′ 0″	13′ 0″
Pitch ,, ...	13′ 6″	13′ 6″	13′ 6″
Draught of water { For^{d}	11′ 8″	11′ 10″	11′ 8″
Draught of water { Aft	14′ 9″	14′ 9″	14′ 9″
Maximum temperature in the boiler-rooms ...	111°	108°	98°
Mean ,, ,, ...	90°	87°	84°

STEAM BLAST TRIALS.

	Heroine.	*Hyacinth.*
Date of trial	May 31, 1882.	July 25, 1882.
Duration of trial... ... ... hours	3	2
Number of boilers used...	2	4
Mean steam pressure in boilers... lbs.	83·9	85·0
,, vacuum ... ... inches	26·0	25·1
,, revolutions per minute	89·4	111·6
,, pressures—lbs. { high	22·45	34·05
,, pressures—lbs. { low	9·63	16·9
I.H.P. { high	310 } 702	586 } 1,445
I.H.P. { low	392 }	859 }
Grate area ... ... square feet	55	110
I.H.P. per square foot of grate... ...	12·76	13·1
Tube surface per I.H.P. ... ... sq. ft.	1·73	1·68
Total heating surface per I.H.P. sq. ft.	2·08	2·02
Wind-force	1	2 to 4
Sea	Smooth	Moderate.
Area of funnel ... ... square feet	*7·5	15
Number and diameter of blast nozzles	Two, $\frac{9}{16}$ inch	Four, $\frac{9}{16}$ inch.
Diameter of screw ... ... feet	13′ 0″	13′ 0″
Pitch of screw	13′ 6″	13′ 6″
Maximum temperature in the boiler-rooms	98°	100°
Mean ,, ,,	87°	88°

* A diaphragm was placed in the funnel for this trial.

"SATELLITE"—FORCED DRAUGHT TRIALS.

Date of trial	May 10, '82	July 5, '82	July 11, '82	July 11, '82	July 11, '82
No. of boilers used	2	4	3	3	3
Duration of trial	3 hours	½ hour	½ hour	½ hour	2 hours
Air pressure ... inches of water	1 to 1½	¼	½	1	1¼ to 2
Mean steam pressure in engine-room lbs.	79	90	86·5	78·7	80·5
,, vacuum ... ... inches	26·6	25·0	25·38	23·88	23·41
,, revolutions per minute	95·4	113 5	103·36	110·66	121·45
,, pressure—lbs. high	27·52	29·95	29·4	33·7	31·93
,, pressure—lbs. low	10·58	12·52	12·85	14·9	14·45
I.H.P. ... high	405	524	469	575	598
I.H.P. ... low	459 } 864	647 } 1,171	605 } 1,074	751 } 1,326	799 } 1,397
Area of fire-grate... ... square feet	55	110	82·5	82·5	82·5
I.H.P. per square foot of grate	15·7	10·6	13·0	16·0	16·9
Tube surface per I.H.P. ... ... sq. ft.	1·41	2·08	1·70	1·37	1·30
Total heating surface per I.H.P. ... sq. ft.	1·69	2 5	2·04	1·65	1·56
Area of funnel ... ... square feet	15	15	15	15	15
Maximum temperature in the boiler-rooms	86°	—	—	—	—
Mean ,, ,, ,,	76°	—	—	—	—

"CONQUEROR"—STEAM TRIALS.

Date of trial...	Jan. 24, '83	Jan. 24, '83	Jan. 26, '83	Jan. 26, '83
No. of boilers used...	8	8	4	4
Duration of trial	2 hours	1½ hour	1½ hour	1½ hour
Air pressure... ... ... inches of water	—	1½	1¼	1¾
Mean steam pressure in engine-room ... lbs.	64	65	61	63
„ vacuum ... ... inches	28·4	27·6	27·8	27·5
„ revolutions per minute	100·8	106	92·21	95·2
„ pressure—lbs { high	27·7	27·3	24·27	25·61
„ pressure—lbs { low	8·27	11·58	6·98	7·48
I.H.P ... { high	2,325	2,408	1,864	2,030
I.H.P ... { low	2,333 } 4,658	3,434 } 5,842	1,801 } 3,665	1,993 } 4,023
Area of fire-grate ... ... square feet	585	585	300	300
I.H.P. per square foot of grate	7·96	10	12·22	13·41
Tube surface per I.H.P ... ... sq. ft.	2·37	1·89	1·55	1·41
Total heating surface per I.H.P. ... ... sq. ft.	2·86	2·28	1·88	1·71
Area of funnel ... ... square feet	64	64	64	64
Maximum temperature in the boiler-rooms ...	100°	102°	110°	116°
Mean „ „ „ ...	73°	85°	91°	98°

PARTICULARS OF THE MACHINERY OF "SATELLITE" AND "CONQUEROR."

	Satellite.	*Conqueror.*
Description of engines	Horizontal compound single-screw, return connecting rod, 2 cylinders	Vertical compound twin-screw, direct acting, 3 cylinders to each set
Diameter of cylinders	H.P. 36″ L.P. 62″	H.P. 54″ Two L.P., each 70″
Length of stroke	2′ 6″	3′ 0″
Diameter of crank shaft	$9\frac{1}{2}$″	$13\frac{3}{4}$″
„ propeller shaft	$8\frac{1}{2}$″	$12\frac{3}{4}$″
Crank pins, diameter × length	10″ × 11″	$13\frac{3}{4}$″ × 12″
Total length of main bearings	5 feet	8 feet each set
Material of shafts	Siemens-Martin steel	Wrought iron
Cooling surface of condensers...	1,900 square feet	9,000 square feet
Air pumps, No. diameter × stroke... ...	One, 14″ × 2′ 6″ Double acting	Eight, 14″ × 3′ 0″ Single acting
Screws, description	2-bladed Griffiths', feathering on Bevis's plan	4-bladed modified Griffiths'
„ diameter × pitch	13′ 0″ × 13′ 6″	14′ 0″ × 16′ 6″
Boilers, No. and description	Four, of long circular type	Eight of high type, 6 flat-sided, 2 circular
„ working load ... ... lbs.	90	70
„ width × length × height	7′ 5″ diameter × 14′ 8″ long	Six, 12′ 2″ × 9′ 6″ × 15′ 0″ Two, 12′ 4″ diam. × 9′ 6″ long
„ thickness of shell plates	$\frac{11}{16}$″	$\frac{15}{16}$″
„ material of	Siemens-Martin steel, except furnaces, combustion chambers, and tube plates	Siemens-Martin steel, except furnaces, combustion chambers, and tube plates
„ No. of furnaces in each, and dimensions of	Two, 2′ 9″ diameter × 5′ 0″ long	Three { 3′ 4″ diam. × 7′ 6″ long-high 3′ 0″ „ × 7′ 6″ „ circular
„ total grate area ... square feet	110	585
„ tubes, material of, and dimensions	Wrought iron, 0·165″ thick, $2\frac{1}{2}$″ diam. × 5′ 8″ between tube plates	Brass, 0·137″ thick, 3″ diameter × 6′ 6″ between tube plates
„ tube surface ... total square feet	2,430	11,050
„ total heating surface, square feet	2,920	13,340
Funnel, description and height from dead plates of lowest furnaces	Lifting, 50′ 0″	Fixed, 65′ 0″
Fans for forced draught, No. and diameter	Two, 5′ 0″	Four, 4′ 0″ aft. Two, 5′ 0″ for^{d}.
Fan engines, description and dimensions	Direct acting, single cylinder, 7″ cylinder × 4″ stroke	Brotherhood, 3 cylinder, 7″ diameter × $4\frac{1}{2}$″ stroke

DISCUSSION.

Mr. F. Marshall: My Lord President and gentlemen, I have read the paper, or have listened to its reading, with very great interest. The experiments recorded (for, as Mr. Butler says, I think we must only regard them as experiments) are certainly of the most interesting nature, and promise to be of very great importance in the future development of marine engineering. The only direction in which we can go with a view to increasing power with the present size of engines, or keeping the engines smaller in order that we may reduce weights, seems to me to be the direction which I ventured to point out at the meeting of the Institution of Mechanical Engineers in Newcastle, and which has been adopted since very generally: I mean the system of forced draught and high speed of movement adopted by Mr. Thornycroft in the working of his torpedo vessels, and which I had the honour of carrying out in designing the machinery for the Chinese cruisers constructed by Sir W. G. Armstrong & Co. There are great difficulties in working this system, and I think Mr. Butler in his paper has pointed them out, or has indicated them at least. These, of course, are really experiments. It appears to me that in carrying out the experiments, much too large grates have been adopted for the work to be done; that had smaller grates been adopted a larger and more satisfactory result would have been obtained. We have no records here of the quantity of fuel burned—that, of course, would have told us whether the grate were large or small in proportion to fuel burnt—but we have a comparison between the grate area and the indicated horse-power. Now I need not tell you, my Lord President or gentlemen here, that to work a 7 ft. 6 in. grate efficiently under such conditions as men have to work in forced draught is a physical impossibility. The back ends of the bars must be almost entirely open, and a very large amount of the air pass off, with the effect of reducing the efficiency of the gases, and cooling the boilers. Therefore, I think in the case of the experiments here, the maximum effect would not by any means be obtained. The grate areas, I think, were much too large. I speak from my own experience. We had very great difficulty in keeping even 6 ft. bars properly covered in the trials to which I allude, conducted in the north of England. There is one very important matter brought out by this paper. It has been a generally received opinion among engineers that the low type of boiler was more efficient than the high type. We are told this morning, and with apparent truth, that the high boilers show very little, if any, inferiority to the low boilers. There is one question I should like to ask Mr. Butler, and that is, whether in putting it at 10 per cent., as he gives it here, of superiority of the low boilers of the *Satellite* over those of the *Conqueror*, he has considered the influence of the pressure. I presume in estimating the efficiency he was working from the indicated horse-power developed. He says they were nearly 10 per cent. better, but it is important to know whether that 10 per cent. was estimated on the pressure of 90 lbs. or on the pressure of 70 lbs., the engines of the *Satellite* having a working pressure of 20 lbs. higher than those of the *Conqueror*. That would, of course, fully account for the difference in the efficiency of the boilers, that is, it would place them almost on a level. There is one point against that conclusion, viz., the *Satellite's* boilers have iron tubes, while those of the *Conqueror* are, I understand, of brass. That circumstance would rather speak in favour of the *Conqueror*, the brass tubes being more efficient than the iron tubes. There is one thing more I would remark. It appears that the maximum amount of power got from the square foot of grate here is about 16½ indicated horse-power. Now, from experiments I have recently conducted with the locomotive type of boiler, I believe there is no reason why we should not obtain 20 indicated horse-power from a square foot of grate. The difficulty of priming seems to have been experienced to a greater extent in the low boiler than in the high boiler. That I cannot quite account for and can scarcely understand,

because the difficulty has generally been to keep the water uniform in boilers of the high class on high speed trials, whereas there has generally been no difficulty with the low type. I may say that in the long series of trials which we had in the three cruisers, extending, I think, over something like twenty high speed trials, lasting from four to eight hours under forced draught, we never were in the least degree troubled with priming, and we generally worked at a pressure of about one inch. Referring again to the question of the high and the low boilers, there is one point in reference to the efficiency of the high boiler under forced draught which is worthy of notice, I think. It will be noticed that in both the natural draught and the steam blast trials, which depend of course upon the efficiency of the funnel, the temperature of the gases was higher in the funnel in both cases in the *Satellite* than it was in the *Conqueror*. This I think we may look for from the mere fact that the gases have a more direct flow through the low boilers than they have through the high, and of course, on the other hand, where the draught is given by fans, we have the lower parts of the boiler under a much more intense heat. In the *Conqueror* boilers, the furnace and back flame-box capacity is greater than in those of the *Satellite*, and present a larger absorbing surface to the intense heat developed in those parts; and further, the resistance to the direct flow of the gases presented by the high boiler as compared with the low, will, I think, account for what we had not expected, namely, the apparently much higher efficiency of that class of boiler when working under forced draught. I should like to observe on this point that the greater efficiency of the heat in the *Conqueror's* boilers over those of the *Satellite* might be balanced in the latter by an increased length of run. It is to be distinctly observed, in speaking of the economy of fuel in connection with forced draught, that everything depends upon the relation of the grate area to the heating surface. Now in both the *Conqueror* and the *Satellite* that relation was as low as 1 to 26; I think in the case of the *Satellite* it was 1 to 23. That is a very low ratio, in fact, it is a natural draught ratio or thereabouts, whereas in the cruisers to which I have referred that relation was about 36 to 1. In the case of locomotives, which may be regarded as our standard in this matter, the relation is about 1 to 80. Now I need not say that in future the locomotive seems to be the form of boiler to which we must look if we are to have small weight for large power. So far as my own experience goes, there is no reason why the locomotive boiler should not be the boiler of the future for, at any rate, war vessels. I have recently made some very excellent experiments with boilers we are now constructing for the Danish Government, in which we have got very high results without the slightest evidence of priming, the steam being blown off under very disadvantageous circumstances. It is quite clear that the most important matter of all to us is that we should be able to reduce our grate areas to the smallest, in order that we may keep them well covered, use higher pressure of air and increased efficiency of fuel. Mr. Butler in his paper mentions that we cannot look for highly economical results from forced draught. I beg to differ from Mr. Butler in that respect. We have in the case of the locomotive generally an indicated horse-power for about 2½ lbs. of coal, —that is not a very highly economic result—at the same time it is a favourable result, and that I think we obtain almost entirely from having small grates and a large amount of absorbing surface and a high air pressure under the grate. The absorbing surface being in the tubes is got with a very small amount of weight. As to the boilers of the *Conqueror*, if you will pardon my saying so, the boilers appear to be about one-third too large for the engines. That, of course, is all right when we are working, as in this case we were working, under ordinary conditions. The engines also seem to have developed very far short of the power which was possible to them. I feel sure that under similar conditions to those used in the cruisers to which I have referred, at least 8,000 indicated horse-power could have been obtained from those engines. That is confirmed by the fact that when working at half power 4,000 indicated horse-power was obtained. I beg to express

my thanks, and I am sure the Institution will do so also, to Mr. Butler, for his very valuable paper, and also I think we may say to the Lords of the Admiralty, for having permitted experiments of such very great value to be laid before this Institution.

Mr. Ravenhill: I should like to ask the writer of this paper a question or two. He was rather pressed for time, and he omitted one or two paragraphs at the end of his paper, notably one where he alludes to the possibility of great improvement in some of our mail boats where the distances from port to port are but short, if forced draught could be introduced. Now at Dover, some years ago, fans were fitted to some of the mail boats there, the object sought being to enable those vessels under all conditions of weather, that is, whether the wind was off the shore or blowing into the harbour, to start carrying their full pressure of steam. One of the first things we did there was to run all our furnace bars together, and the trials were abandoned. I would like him to tell us if he can, the condition of the fire-bars after the trials on board the *Conqueror* and on board the *Satellite*, because in the paper he quotes very high temperatures indeed as having been registered in the uptakes of the boilers on both those vessels. There is yet, no doubt, a great deal to be learnt, and probably the principal point to be learnt will be found to be how the boiler will stand the increased work to which it will be subjected. The advantages, of course, if it could be carried out on short distance stations would be very great indeed, because the difference between the ordinary draught and the forced draught, as described by the writer of the paper, say on the Folkestone station, would produce something very like an increase of a knot an hour in the speed of the fastest vessels on that station. This would be an enormous gain, and anything that Mr. Butler can supplement this paper with, with reference to observations made on the condition of the boilers at the conclusion of the trials, would add vastly to its interest. I am quite sure the Institution owes to him and to the Admiralty a deep debt of gratitude for having given us these their first experiments. It has been suggested that I should ask Mr. Butler what state the uptakes and the chimneys were found to be in at the conclusion of the trials after they had been subjected to this very high temperature of, say, 1,000 to 1,200 degrees Fahr.

Mr. Robert Humphrys: My Lord, I do not agree with Mr. Marshall or Mr. Butler in the statement that a 7 ft. bar is too long to be worked efficiently. In the case of the *Conqueror* it was 7 ft. 6 in., and on the trial we certainly had fluctuations in the air pressure in the stokeholes, but in my opinion that was due to the fact of the difficulty we had in making the stokers realise that they were burning 40 per cent. more coal than they usually did with the steam blast, but towards the end of the trial the bars were thoroughly well covered, and we were getting the full effect from them. Mr. Marshall states, also, that he thinks that we should reduce our grate areas to the smallest, in order that we may use high air pressure. I venture to differ entirely with him there, because if you work with high air pressure it is more difficult to manage, and your bars get dirtier sooner, because they burn a greater quantity of coal in a given time. Mr. Marshall has also told us that he has obtained in the locomotive boiler 20 horse-power per foot of bar. I have no doubt a great many members here will be very glad if he will tell us under what circumstances he did that. Unfortunately, we have had some experience of locomotive boilers, and we have not exactly been able to do it. Making experiments in one's own yard and making experiments on board ship I find are totally different things. Captain Noel in his paper read yesterday stated that he was afraid that enclosed stoke-holes would lead to a scare amongst the stokers if any sudden rush of water were to come in. I would like to point out that the arrangement carried out in the *Conqueror* is really safer than an ordinary stoke-hole, because men could get out by proper steps fitted on the bunkers through the doors in the covering plates, and then through the double doors of the engine-room.

In the event of steam-pipe or boilers being damaged the men would be practically safe, because they have an outlet into the engine-rooms through the double doors. Mr. Butler comes to the conclusion that as the air pressure increases the consumption of fuel proceeds at a much higher rate than that of the power given to the engines. Consequently, I think his ideas would coincide with mine, that the better plan would be to lower the pressure.

Mr. R. H. Andrews: My Lord, I think it will be very interesting to the meeting, if Mr. Butler will kindly tell us what was about the thickness of the fires in the *Satellite's* and *Conqueror's* boilers during these trials. We are inclined to associate the results of these experiments with those of the locomotive type of boilers, where we find the fires varying in thickness from 14 inches to 2 feet, and in some cases more than that. When working with forced draught, it is absolutely necessary to have the fires much thicker than with ordinary (natural) draught. The *Conqueror's* furnaces are only about 3 ft. 4 in. diameter, and assuming the dead plate to be about two inches above the centre, it only leaves about 1 ft. 6 in. from the top of the bars, in front, to the top of the furnace, and if there is a thickness of fire of only 1 foot at the beginning of the bars, which is practically at the mouth of the furnace, how is it possible for the stokers to put back coal enough at the end of 7-feet bars to keep them well covered? I must, therefore, beg to differ from Mr. Humphrys that these 7-feet bars can be kept sufficiently well covered for working with forced draught. Long bars cannot be so efficiently fired as short ones, and I find, from many years' experience with torpedo-boat boilers, that it is practically impossible to fire efficiently bars more than 5 ft. 6 in. long; that is, to maintain a thickness of fire up to 14 inches, and that a stoker has quite as much as he can do to fire a furnace of 25 square feet in area, when burning 80 to 90 lbs. of coal per square foot of fire-grate per hour, to keep the bars well and evenly covered; and if they are not kept well covered I am sorry to say we know too well that the tubes will soon begin to leak. This is the case with the locomotive type of boiler, and probably would be also with the *Satellite's*, where the flame impinges directly on to the mouths of the tubes. I think that in designing boilers of the *Satellite's* and *Conqueror's* type, to be worked under forced draught, that we must go in for much larger furnaces, say, 4 feet in diameter. This will cause a corresponding reduction in the length of the bars, and admit of their being fired efficiently. It will also be interesting to the meeting to know what was the pressure of air in the furnace, in the combustion chamber, in the smoke-box, and at the root of the funnel in each ship, so as to compare the loss of pressure due to resistance in each type of boiler. The quantity of coal burnt in each type of boiler cannot be compared by the pressure of air in the stokehole, as that pressure depends upon the resistance the air meets with in passing from one part of the boiler to another. In the *Satellite's* boilers the products of consumption go almost direct into the tubes from the furnace, and from the smoke-box straight up the funnel. This is similar to that of the locomotive type, but in the *Conqueror's* boilers the gases have to turn up from the furnace into the combustion chamber, then turn again at right angles into the tubes, and a third turn from the smoke-box into the funnel. We should, therefore, expect to find a great difference of air pressure in the stokeholes of these two ships when burning equal quantities of coal per square foot of grate per hour in the same sized furnace, and, consequently, the air pressure is no measure of the consumption of coal; nor can the consumption be compared by the air pressures used in different types of boilers, only in boilers of the same or similar types. I find with the ordinary locomotive type of boiler in torpedo boats, that after steaming, say an hour, at full speed, the air pressure increases considerably, but there is no more coal being burnt then than at the beginning; it is simply due to the increased resistance to the air and gases owing to the fire-bars, tubes, &c., getting dirty and partially choked. I would

like to call attention to one thing in connection with this paper. We have been speaking of the "consumption of coal," but I think it would be far better to call it the amount of "coal expended." It is well known that we do not get so much benefit out of the coal used as we ought to when working under forced draught, and that in boilers of the locomotive and *Satellite* type a great quantity of coal is blown into the smoke-box and up the funnel in a partially consumed state, and at present there does not appear to be any means of avoiding this waste. I therefore think that under such conditions "coal expended" is a much more appropriate term than "coal consumed."

Mr. J. Wright: My Lord, will you allow me just to say a few words before you close the discussion? Attention has been called to the very good behaviour of the high boilers in the *Conqueror* as compared with the low boilers in the *Satellite*, and I think, if Mr. Marshall will look at the drawing, where the boilers are not shown in detail, he will see as reasons for this that there is a very large amount of steam space, and the water spaces are also very good. Another thing Mr. Butler mentioned in his paper, although he did not read it, was that the feeding in the *Conqueror* was done by a separate feed-engine, and that the feeding was remarkably regular. We know that feeding from the main feed-pumps is very often irregular and spasmodic when working at high powers, and very often leads to priming. In this case the feeding was everything that could be desired. I am afraid that Mr. Humphrys is very much in the minority with regard to length of fire-bars. When the *Conqueror's* bars were shortened to 6 ft. 6 in., the results obtained were certainly, I think, better than with the longer bars, and the increase of air pressure that may be required to be given to fire-bars of moderate length that can be well worked would be very little. I am glad Mr. Humphrys has called attention to the comparative safety of the enclosed stokehole. It is a point that Captain Noel alluded to yesterday as a very serious matter, but I think that when men once get accustomed to it they will really prefer working in a closed stokehole to an open one. It is certainly more comfortable, if you do not stand immediately under the fans; it is cooler and more comfortable in every way. Mr. Butler mentions in his paper that our neighbours, the French, have been ahead of us in this matter of applying forced combustion in closed stokeholes to ordinary boilers. The experiments that have been made here as yet are, in a measure, preliminary, and only to a certain extent of value. They are not of much value to the commercial marine at the present time. They would have been more so, I dare say, if we could have had given the coal consumption, but I am afraid the effect of that would have been, upon those, at all events, who saw the rapid rate at which the coal went, rather to deter them from adopting the system. What I was going to say was this, that our good neighbours, the French, having gone ahead of us in this matter, we should be very glad if our distinguished member, M. de Bussy, would favour the Institution, at some future time, with some results of their trials under forced combustion.

Mr. Butler: There are not many points, my Lord, I think, I have to allude to, and I will not detain the meeting long. Mr. Marshall spoke of the long grates of the *Conqueror's* boilers being, in his opinion, unworkable. I must say I agree with him, and I must differ from Mr. Humphrys in that respect. I was present on the trials made in the *Conqueror*, when these long grates were fired, or attempted to be fired—because it cannot be said that they were fired efficiently when every now and then a rake had to be put into the furnaces to level the fires. The furnace doors had to be kept open for some considerable time, and thus large volumes of cold air at considerable pressure were driven straight in on to the backs of the tubes. With regard to the priming that I made reference to as being observed in the boilers of the *Satellite*, I endeavoured to make it clear that this was

probably only apparent because no water was observed to come over into the engine-room; but the effect it had on the people in the boiler-room—the men in charge—was just the same as if it were real and the water had come over, because the disappearance out of the glasses rendered it necessary for safety's sake for the feed-engines to be put on to make up the apparent deficiency. With regard to the *Conqueror's* boilers, it must be recollected that they were designed to produce steam for the engines only sufficient for the contract power when worked with the steam blast, and therefore, when they are worked with the air pressure, they appear to be extremely large in proportion to the size of the engines. Then as for the question of lengthening the tubes in these boilers, so as to get a better result and a less temperature in the uptakes, it must be remembered that the boilers of these ships were, and for war ships must necessarily be, designed primarily to enable them to meet the requirements of the ordinary work of the ship at all times, and the application of the forced draught in them will be very exceptional; in fact, it is not intended that the forced draught shall be applied except in cases of emergency or occasionally for a short time to enable the men to be trained in its use. Mr. Ravenhill asked what condition the fire-bars of the *Conqueror* and *Satellite* were in after these trials, and also the uptakes and chimneys. In the *Satellite*, in fact in both these vessels, the fire-bars are made of wrought iron, about 3½ inches deep. No effect was observable on the fire-bars at all, but the fire bridges of the *Satellite* were found to be slightly burned; no ill effect was observed on the uptakes and chimneys of either of these vessels. Mr. Andrews wished to know what thickness of fires we kept in the *Satellite* and the *Conqueror*. In the *Satellite*, so far as I can recollect, it was about 7 inches, and in the *Conqueror* we attempted to keep 10 inches, and no doubt 10 inches were kept on the front bars, but it is very doubtful whether that thickness was maintained uniformly. I do not think there was any other point, my Lord, that called for any remark from me. I beg to thank the meeting most sincerely for the manner in which my paper has been received.

Mr. W. H. White: Will your lordship allow me for a moment? M. de Bussy will not rise himself, although I have been asking him to do so, but he asks me to be his mouthpiece, and to say that he will have the greatest pleasure in furnishing the Institution with the results of the best French experience which he can put at the service of the Institution.

The President: Gentlemen, I am quite sure you will allow me to express a double vote of thanks on this occasion; first of all to the writer of this most interesting, and, I venture to think, one of the most important papers which is likely to be read—to Mr. Butler, for the reading of that paper, and to our distinguished and honoured friend, M. de Bussy, for the last announcement that he has made. I dare say you will pardon me a moment if I make an observation or two upon the discussion. I regret extremely, gentlemen, and I have no doubt many of you will regret with me, that no estimate has been given, apparently for some very good reasons, of the quantity of coal burned, because really (I speak with some little knowledge upon this subject, because I live in a smoke country, and we all know that smoke is practically wasted fuel, and those who live in a smoke country know what they suffer in consequence) economy of fuel is the great object that we are all seeking to obtain by the introduction of improved boilers and furnaces. In the long voyages it becomes a question of enormous magnitude, and the saving of fuel is really one of the greatest questions at the present day in that view. There is another point, and that is this, that all the study of all our highly-instructed engineers is of very little use, and is really thrown away in the improvement of this machinery, if those who have to use it are unable to use it properly. And that leads me to the question of stoking, which is one of the most important questions

of all. How we are to secure at all times in the Navy not only a sufficient supply but a reserve of highly-trained stokers, is really one of the most important questions that can be dealt with as a national question. I merely wished to touch upon this subject, and I am very much obliged to Mr. Andrews for that important remark which struck me at the time when he called attention to the question of smoke, which, as I said before, in practice really means wasted fuel. I am sure you will allow me to return jointly to Mr. Butler and M. de Bussy your united thanks.

THE INFLUENCE OF THE BOARD OF TRADE RULES FOR BOILERS UPON THE COMMERCIAL MARINE.

By J. T. Milton, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

At our last Annual Meeting I had the honour to read a paper upon "Lloyd's Rules as affecting Marine Boiler Construction." In the present paper I wish to draw the attention of this Institution to the results of the action taken by the Board of Trade in regard to Marine Boilers; and as I shall have to compare in some respects the Rules of the Board of Trade with those of Lloyd's Register, permit me to say at first that I do this with no spirit of rivalry between the two bodies, for no rivalry is possible between a Government Department, compelling their rules to be adopted in all steam vessels engaged in trades requiring them to have passenger certificates, on the one hand, and a voluntary institution like Lloyd's Register on the other, in which it is open to any shipowner not to have his vessel classed, or to withdraw her from classification the moment that Lloyd's Rules are found to prejudice his business in any way; but I refer to Lloyd's Rules as giving results which are well known to all persons interested in marine boiler construction and maintenance.

To give some idea of the great influence which the Board of Trade Rules must have upon the commercial marine, I may mention that at the present time there are nearly 1,000 sea-going vessels, of an aggregate gross tonnage of nearly 2,000,000 tons, and of nearly 300,000 registered nominal horse-power, holding passenger certificates; and this amount is increasing rapidly; for out of the large number of new steam vessels registered in the United Kingdom in 1882, 38 per cent. of the tonnage, and 45 per cent. of the nominal horse-power, came under the Board of Trade Rules, there being in that year about 114 new sea-going vessels built under these Rules, having a gross tonnage of over 295,000 tons, and an aggegate nominal horse-power of over 41,000.

Seeing what a large proportion of British steam vessels are compelled by law to come under the periodical survey of the Officers of the Board of Trade, and to have their boiler pressures fixed by the Board's Rules; and recognising that these vessels are built with the idea of their producing a commercial return upon their cost, and that in all cases

this return is largely, and in some cases wholly, dependent upon the economy of their fuel consumption, which is itself governed in a large measure by the steam pressures the boilers are permitted to carry; it is essential that the rules by which the Surveyors are guided in fixing these pressures should be framed upon an equitable basis, which, while providing for absolute safety, should in all cases allow the greatest pressures to be carried which are consistent with safety.

It is a comparatively easy matter to calculate the strain produced by any given steam pressure upon the material of which a boiler is constructed, when we know its dimensions and scantlings, but it is experience, and experience alone, which can determine the greatest amount of strain which may be put upon a material and still leave a sufficient margin of strength to secure absolute safety. Unfortunately, the only direct experiences which give these data regarding boilers are to be found in those cases in which signs of weakness make themselves apparent, and, as the results of a failure of even a small part of a boiler would generally be very disastrous, such experiences are never sought after, and, happily, very rarely occur; but, on the other hand, we have very many experiences of a negative character, each showing, by the fact of the boilers working continuously with a given pressure safely, that the limiting safe strength is at least not less than that at which the boilers work. As a matter of fact, there are considerably more than 3,000 steamers classed by Lloyd's Register, the boilers of which are working, and have worked safely for years, at pressures greatly in excess of those which would be allowed by the Board of Trade Rules, while the practice with all foreign nations is usually to make the boilers still lighter than recognised by Lloyd's Rules.

In comparing the Board of Trade Rules with those of Lloyd's Register, it must be distinctly remembered that the former are intended to apply to all boilers coming under survey, whether the boilers be new or old, and that the longest period for which a passenger certificate is granted is twelve months, while Lloyd's Rules are intended to apply to *new* boilers only; and seeing that vessels with new boilers are classed and retain their class, even if their boilers do not become re-surveyed for four years, and that the majority of them do not become re-surveyed within this period, it is evident that Lloyd's Rules for new boilers must provide an allowance for corrosion, and consequent deterioration of strength, greater than would be required if the boilers were only to be passed for twelve months.

So far, however, from the Board of Trade Rules permitting of higher pressures in boilers passed for one year only than those for which the same boilers would be passed by Lloyd's Register for four years, the rules of the latter body permit of pressures of from 10 per cent. to 25 per cent. greater being carried than would be allowed by the Board of Trade. Further, of the hundreds of steam vessels coming under the inspection of Lloyd's Register, with their boilers four years old, although their strength must have then become somewhat reduced, in most cases they are found to be sufficiently

strong to be passed for a further period of two years with their original working pressure, and in very many instances are boilers eight years of age and upwards originally constructed no stronger than now required by Lloyd's Rules, working safely, without having had their pressures reduced.

Not only, however do the Board of Trade Rules insist upon a less pressure being carried in all cases than it would appear could safely be allowed, but these Rules, by not being based upon a sound system, severely handicap some particular methods of construction as compared with others, and thus fail to inspire confidence in Marine Engineers.

For instance, on referring to the rules for cylindrical shells, we find :—

"It has been represented to the Board of Trade that boilers well constructed, well designed, and made of good material, should have an advantage in the matter of working pressure over boilers inferior in any of the above respects, as unless this is done the superior boiler is placed at a disadvantage, and good workmanship and material will be discouraged. The Board of Trade Surveyors have endeavoured for some time to take all these points into consideration in fixing pressure, and for this purpose the following Rules were prepared, and, at the request of engineering firms, subsequently circulated.

"When cylindrical boilers are made of the best material, with all rivet-holes drilled in place, and all the seams fitted with double butt straps each of at least five-eighths the thickness of the plates they cover, and all the seams at least double riveted, with rivets having an allowance of not more than 75 per cent. over single sheer, and provided that the boilers have been open to inspection during the whole period of construction, then 5 may be used as the factor of safety. The tensile strength of the iron is to be taken as equal to 47,000 lbs. per square inch with the grain, and 40,000 lbs. across the grain. The boilers must be tested by hydraulic pressure to twice the working pressure, in the presence, and to the satisfaction, of the Board's Surveyors. But when the above conditions are not complied with, the additions in the following scale must be added to the factor 5, according to the circumstances of each case :—

A	·15	To be added when all the holes are fair and good in the longitudinal seams, but drilled out of place after bending.
B	·3	To be added when all the holes are fair and good in the longitudinal seams, but drilled out of place before bending.
C	·3	To be added when all the holes are fair and good in the longitudinal seams, but punched after bending instead of drilled.
D	·5	To be added when all the holes are fair and good in the longitudinal seams, but punched before bending.
E*	·75	To be added when all the holes are not fair and good in the longitudinal seams.
F	·1	To be added if the holes are all fair and good in the circumferential seams, but drilled out of place after bending.

G	·15	To be added if the holes are fair and good in the circumferential seams, but drilled before bending.
H	·15	To be added if the holes are fair and good in the circumferential seams, but punched after bending.
I	·2	To be added if the holes are fair and good in the circumferential seams, but punched before bending.
J*	·2	To be added if the holes are not fair and good in the circumferential seams.
K	·2	To be added if double butt straps are not fitted to the longitudinal seams, and the said seams are lap and double riveted.
L	·1	To be added if double butt straps are not fitted to the longitudinal seams and the said seams are lap and treble riveted.
M	·3	To be added if only single butt straps are fitted to the longitudinal seams, and the said seams are double riveted.
N	·15	To be added if only single butt straps are fitted to the longitudinal seams, and the said seams are treble riveted.
O	1·0	To be added when any description of joint in the longitudinal seams is single riveted.
P	·1	To be added if the circumferential seams are fitted with single butt straps, and are double riveted.
Q	·2	To be added if the circumferential seams are fitted with single butt straps, and are single riveted.
R	·1	To be added if the circumferential seams are fitted with double butt straps, and are single riveted.
S	·1	To be added if the circumferential seams are lap joints and are double riveted.
T	·2	To be added if the circumferential seams are lap joints, and are single riveted.
U	·25	To be added when the circumferential seams are lap, and the strakes of plates are not entirely under or over.
V	·3	To be added when the boiler is of such a length as to fire from both ends, or is of unusual length, such as flue boilers; and the circumferential seams are fitted as described opposite P, R, and S, but of course when the circumferential seams are as described opposite Q and T, V ·3 will become V ·4.
W*	·4	To be added if the seams are not properly crossed.
X*	·4	To be added when the iron is in any way doubtful, and the Surveyor is not satisfied that it is of the best quality.
Y	1·65	To be added if the boiler is not open to inspection during the whole period of its construction.

"Where marked * the allowance may be increased still further if the workmanship or material is very doubtful or very unsatisfactory."

"The strength of the joints is found by the following method :—

$$\frac{\text{Pitch} - \text{Diameter of rivets} \times 100}{\text{Pitch}}$$

= percentage of strength of plate at joint as compared with the solid plate.

$$\frac{\text{Area of rivets} \times \text{No. of rows of rivets} \times 100}{\text{Pitch} \times \text{thickness of plate.}}$$

= Percentage of strength of rivets as compared with the solid plate.†

"Then, take iron as equal to 47,000 lbs. per square inch, and use the smallest of

† "If the rivets are exposed to double sheer, multiply the percentage as found by 1·75."

the two percentages as to the strength of the joint, and adopt the factor of safety as found from the preceding scale :—

$$\frac{47{,}000 \times \text{percentage of strength of joint} \times \text{twice the thickness of the plate in inches}}{\text{Inside diameter of the boiler in inches} \times \text{factor of safety.}}$$

= pressure to be allowed per square inch on the safety valves."

This scale seems to provide for all possible methods of constructing cylindrical boiler shells, and if the additions to the factor of safety made by it were such that the ratio of the ultimate strength to the working pressure allowed was in all cases the same, no objections could be raised to the scale. This, however, is far from being the case.

It is generally received that the strength of a cylindrical boiler shell to resist an internal pressure is dependent upon the strength of the material and of the proportions of the longitudinal seams. The pressure on the ends of the boiler being mostly borne by the stays, causes very little strain upon the circumferential seams, and so long as these seams are tight no more is required of them ; the design of these seams, whether lap, single strapped, or double strapped, single or double riveted, with punched or drilled holes, having absolutely no effect upon the ultimate strength of the shell. There are, however, no less than six provisions in the scale as to the design of these joints, and five as to the method of making the holes in them, some of these provisions involving a possible reduction of 12 per cent. in the working pressure. Curiously enough, although these eleven provisions are made, no notice whatever is taken of the proportions of the diameter and pitch of the rivets in regard to the thickness of the plates, the most important points so far as the efficiencies of these joints are concerned.

With regard to the longitudinal joints, there can be no doubt that if the holes are punched the plate is weakened to a somewhat greater extent than it would be if they were drilled, and possibly the plate may be left a little stronger if it is holed after bending than if holed before bending; but there can surely be no justification whatever for the provision A in the above scale, the holes in each case being required to be fair and good and drilled after bending. When we consider the rivets, however, experience has shown that rivets in punched holes are certainly not weaker than those in drilled holes; yet these rules, by requiring the same sectional area of rivet as net section of plate in all cases, or by reducing the pressure in proportion as the rivet area falls short of this, allow a less strain to be borne by rivets in punched holes than by those in drilled holes of the same size: a difference of 10 per cent. being made in this way by the provision D.

It would be impossible, in the limits of this paper, to exhaustively criticise the whole of these twenty-five conditions. I will here only further remark, that the condition V victimises double-ended boilers to the extent of from 6 to 7 per cent.

The Board of Trade Rules for furnaces, again, give a variety of different pressures for flues of the same diameter, length, and thickness, according to the method of con-

structing the longitudinal joints, a furnace with a welded seam being allowed to work at a pressure 50 per cent. in excess of that which would be allowed if the joint were lapped and single riveted with punched rivet holes. In these Rules, as with those referring to the circumferential joints of cylindrical shells, the method of making the holes in the plate is deemed to be of great importance, while the proportions of diameter and pitch of rivets to the thickness of the plates are again ignored.

Now, with the same dimensions and furnaces, and with the same strength of materials employed, it is well known that the strength of the structure to resist collapse depends upon the *roundness* of the flue, and not upon the method of joining or holeing the plates. Actual measurements of hundreds of flues, constructed by all the Engineering firms throughout the country, have been taken by Lloyd's Surveyors in the ordinary course of their duties, and the results show that not one method of making the joints gives invariably better results as regards roundness than others, some of the flues most out of truth being welded or fitted with double-butt straps.

Lloyd's Rules with regard to furnaces would permit in some cases of more than 70 per cent. greater pressure being carried than would be allowed by those of the Board of Trade; and in no case yet have Lloyd's Rules, out of the thousands of boilers made in accordance with them, been proved to be too light, the whole of the cases of collapse of furnaces which have occurred being clearly due to overheating from various causes.

The Board of Trade Rules in respect to furnaces do not absolutely prevent high pressures being carried in the same way as their Rules for shells, because the strength of a furnace can be increased without thickening the furnace plates by fitting anti-collapsing rings, but all Engineers who have the responsibility of keeping in order boilers fitted with these rings, feel so strongly upon this point, on account of the anxiety and trouble these rings incur, that nothing but sheer necessity can induce them to have them fitted.

Again, the Board of Trade Rules for thickness of flat plates, assume their strength to vary directly as the square of their thickness in sixteenths + 1, and inversely as the surface supported —6 square inches,—a law which cannot possibly be true. Moreover, if the co-efficients employed are suitable for the pitches and pressures usually employed with thick plates, this rule must give results dangerously weak when applied to thinner plates with smaller pitches; while, if the co-efficients are suitable for the thin plates, it must seriously reduce the pressures allowed to be borne by thick plates. If any difference in point of original strength is required in the cases of thick *v.* thin plates, surely the thin ones should be made relatively stronger, since they would be more weakened than the thick ones by an equal amount of corrosion.

These Rules for flat plates, like those for furnaces, do not absolutely prevent high pressures being carried, since it is possible to meet them by pitching the stays very closely together; but this close pitching of the stays, like the fitting of anti-collapsing rings to

furnaces, seriously interferes with the continued efficiency and safety of the boiler, by rendering it extremely difficult, and in some parts impossible, to obtain access to the various portions of the boiler for purposes of inspection, cleaning, and repair. The Rules for shells, however, offer a complete obstacle to further progress in Marine Engineering, as the thickness of plates now rendered necessary by them to enable existing pressures to be carried is as great as can be properly closed and riveted together with our present appliances; and although powerful hydraulic riveters are employed for this work, it must be remembered that with our present form of boiler it is necessary for at least one circumferential seam to be riveted by hand.

In the case of steam vessels built under Board of Trade inspection, and originally intended to receive a passenger certificate, the hardship of these Rules, although serious, is not so directly felt as in those cases in which existing steam-vessels, through changes of commercial activity, the opening of new trades, &c., require to carry more than twelve passengers; because in the one case the Owners from the first have been aware of the disabilities to which they would be subjected, and the machinery has been designed to work at the relatively low pressures; but to the Owners of the other vessels it must appear to be not only absurd, but also despotic, that the boilers will only be allowed to carry a fraction of the steam pressure for which the machinery has been designed, and for which both boilers and engines have proved to be perfectly safe. A large reduction of pressure in a compound engine proportioned for a high pressure means a considerable loss of efficiency, and in the case of low-powered steamers it may often happen that this reduction of pressure and power, instead of rendering the vessel more safe, may, by rendering her less manageable, actually tend to increase her chances of succumbing to heavy weather.

It will be within the recollection of many Shipowners and others here that a few winters ago considerable anxiety was felt as to the safety of many steamers then engaged in the Atlantic trade, solely on account of their being of low power.

I have referred, in the early part of this paper, to some figures showing the immense interest this country has in the Steam Commercial Marine, and have shown that the Board of Trade Rules press unnecessarily heavily upon its prosperity. It is very difficult to estimate the monetary loss annually inflicted upon the country by these Rules, but assuming that the average steaming time of all sea-going passenger steamers is 250 days per annum, and that the boilers of these vessels are capable of being worked safely at pressures 20 per cent. greater than those actually allowed, and that this 20 per cent. increase of pressure would give about 4 per cent. increase of efficiency, figures which are all well within the mark, then at least 300,000 tons of coal are burned per annum in these passenger steamers wastefully.

Assuming that the average cost of this coal is £1 per ton, and that the average loss in freight through having to carry this coal instead of cargo is £1 per ton, we see that these Rules cost the country more than half a million pounds per year.

Having pointed out some of the great hardships at present inflicted by these Rules, I can only, in conclusion, express the hope that the President and Council of this Institution may agree with me that this is a subject in regard to which they might with great public advantage bring their influence to bear upon the Authorities of the Board of Trade, with a view to having these Rules placed upon a satisfactory basis; and, if I may make a suggestion on this point, it would be to the effect that in revising the Rules, those Engineers of eminence, who have had large experience not only in making but in the upholding and running of marine machinery, should be invited to give their experiences, as these would be of the greatest value in framing Rules for minimum strength.

DISCUSSION.

Mr. MacFarlane Gray: My Lord and gentlemen, I need not tell you that I do not represent the Board of Trade. I know very little about the Rules. The Rules belong to another department in the Board of Trade than that with which I am connected; and where there are different departments in a concern, it is sometimes better for those of one department not to pay too much attention to the affairs of another department. I myself, therefore, have not paid very much attention to these Rules, and if I this morning make any mistake in interpreting them, you must forgive me. The remarks that I have to make, then, are simply those which have occurred to me as one having a little knowledge of these things. Looking at Mr. Milton's paper, it seems to me that it expresses the influence of the Board of Trade Rules upon Lloyd's Surveyors more than upon the Commercial Marine. If you turn with me to the paragraph beginning: "It is generally received"—(this is the first paragraph containing his attack)—"that the strength of a cylindrical boiler shell, to resist an internal pressure, is dependent upon the strength of the material and of the proportions of the longitudinal seams." This, then, is to be the basis of our Rules, if this body is established to make new Rules, and if Mr. Milton has his way. Now, is this the case? It seems to be so, by describing a circle and by drawing a boiler longitudinally, that the circumferential seams are, in every case, if the joints are the same, double the strength of the longitudinal, that is, that they have 50 per cent. to spare. If you ask the boilers themselves, what do they say? Are they not to have any voice upon the matter? I wish I could have taken you with me to a dock in London a few years ago—I am sure Mr. Parker saw it—where you could have seen the boilers of the screw steamer, the *Thames*. In those boilers a circumferential seam went—I am within an inch or two of it—in one instance, say 100 inches; there was a seam-rip of 100 inches right across the boiler. Now, how would Mr. Milton explain that? Did any of us ever hear of a longitudinal seam of a boiler having 100 inches of seam-rip? Where would the boilers have been if they had been at sea under the normal pressure? Fortunately, on this occasion the pressure was only 17 lbs. to the square inch instead of 70 lbs. If it had been 70 lbs.—and it might come about to be 70 lbs. and that very thing to happen—where would that boiler have been? Now, I think this ought to dispose very fully of this statement that the strength of a boiler shell is dependent solely upon the strength of the material and of the proportions of the longitudinal seams. I say that it is not; that practically the circumferential seams of a boiler are far more strained than the others, as any man knows who has examined the bottom of a boiler and has seen where the leaking goes

on. It is the circumferential seams, it is not by the hydrostatic pressure, nor the normal uniform pressure all through. It is the unequal heating of the parts of the boiler, and the boiler must be made strong enough for that. You may say, "Well, that is only one case." Only one case! Well, you would not require to go very far to find another. While the engineer was looking at this one, he could not understand it. He had heard a strange noise: he thought someone had got a box of safety matches and was striking them all round him. He heard the noise and could not make out where the sound was coming from, until he saw the boiler actually opening before him. Then again he told me, "It was just as if another fellow had got another box of matches behind my back and was striking them there: it was the other boiler going the same way." That was his account of it. He found it was the other boiler going the same way at his back. It went at 17 lbs. pressure. Now, the cause of that was that they had run in some cold water at the bottom of the boiler; they had run it in from the sea. They were coating the boiler with cement and heating the boiler to dry it, and had opened the sea-cock and allowed the water to be run in from the sea. They, therefore, had a body of cold water lying in the bottom of the boiler, and by and by when the pressure rose to 17 or 18 lbs., away went the boiler in that way. Now, that did happen there, but you may say it could not happen at sea. We did not expect that to happen there. We do not provide specially—we cannot, I suppose—for everything we have to meet in regard to making boilers strong enough. Such an accident might occur at sea in this way: If one of a set of boilers has been for some reason or other laid off, and it is connected again, and if some fool opens the stop valve, the communicating valve between the two boilers, while a large quantity of cold water is lying at the bottom of the boiler, and lets in at once the full pressure at which it is intended to work, where will the boiler go? It has not happened yet, and these boilers are said to be quite safe. I know they are perfectly safe until some accident of this kind happens. We could all name boilers that were perfectly safe, and believed to be perfectly safe, until there was something about the opening of a stop valve, or a rivet bag left in a stop valve, or something else of that sort, which caused an accident. Now, we cannot tell when the mistake will happen, but that has happened already, and I say that I have shown, I think, from that one example, that it is quite a mistake to make that statement that the strength of a boiler depends upon its longitudinal seams only, and that therefore you need make the circumferential seams only half the strength. I do not dispute at all that the strength one way against internal pressure is double the strength of the other. The longitudinal seams have equally over the whole length about the same strain, whereas the circumferential seams may have even almost compression on one side of the boiler and a very inordinately increased amount of tension upon the other. Now, that must have been the case with regard to that boiler of the *Thames*. Mr. Milton ridicules your having these provisions for the circumferential seams. Of course, I do not know whether Mr. Milton is making a mistake to suppose that the additions for the circumferential seams are to be added after the corresponding additions for the longitudinal seams have been added.

Mr. MILTON: They are.

Mr. MACFARLANE GRAY: They are not at all. The Rules do not say that the one is to be added to the other.

Mr. MILTON: Read the Rule.

Mr. MACFARLANE GRAY: It is simply insulting the Surveyors to imagine that they would do it. If you are making an allowance for a chafed rope, and if you find that at a certain part of the rope there is, say, 20 per cent. of it gone, and you make an allowance for that 20 per cent., then in

passing over the rope, you find another part of it where it is 10 per cent. gone, Mr. Milton fancies that they are so foolish in the Board of Trade that they would add the two and say—"That is 30 per cent." That is nonsense; they do nothing of the sort.

The President: Mr. Milton will have the opportunity of replying, but I must ask you to be a little cautious in the phrases you use.

Mr. MacFarlane Gray: I should be very sorry if I have said anything wrong or out of taste. He says in the paper, "Curiously enough, although these eleven provisions are made, no notice whatever is taken of the proportions of the diameter and pitch of the rivets in regard to the thickness of the plates." I am not aware of that. I think they are the usual proportions of rivets to plates, and the Rules distinctly tell that where there is any boiler with unusual proportions the surveyor or the builder has to apply to the Board, and that unusual proportions come under a different Rule altogether. I think that is about all in that paragraph that I need notice. Then in the next paragraph we have this: "With regard to the longitudinal joints, there can be no doubt that if the holes are punched the plate is weakened to a somewhat greater extent than it would be if they were drilled." He says: "Possibly the plate may be left a little stronger if it is holed after bending than if holed before bending; but there can surely be no justification whatever for the provision A in the above scale, the holes in each case being required to be fair and good, and drilled after bending," and he goes on to complain of the 10 per cent in D. That refers to the provision for holing or punching before bending. Now, remember, I have nothing whatever to do with these Rules—nothing whatever. I do not know how they were made, but the remark I make will, perhaps, explain why they are so particular about this, against bending after holing. I remember that about that time a surveyor, in inspecting the plates in a first-class boiler-maker's place where they were making a lot of very large boilers, looked at a heap of plates in the yard. He said to the foreman, "Are these condemned plates?" "No; these are not condemned plates." "Why, they are cracked at that seam!" "Cracked! Nothing of the sort! we have sent back four or five plates already, but these are all right." Now, on examining the plates it was found that by laying the plates outside, the exposure to the weather had shown, what had escaped the strictest examination before, that several of the plates were gone this way [describing on the board]. The plates were an inch and an eighth in diameter. Taking one of the holes, *there*, that plate had been bent after being punched, and it was cracked between the holes for a dozen holes in succession, and quite a number of plates were found in the same way. I have no doubt that many plates of that kind are put into boilers and the defects not seen at all. Now see what happens to it. They put on a covering strap, or put another plate over that—and remember this one is cracked—and then you put in your rivet, and that is where the crack is, and I have Mr. Milton's own word for it that the strength depends upon the longitudinal seam. *That* is the longitudinal seam. The crack is half through because it was cracked by being bent after being punched. Now, I think that that would be an element of danger in a boiler, and I know that it was a matter of serious consideration with regard to boilers at that time by the Board of Trade. I think they therefore demand from you some consideration for having made, what Mr. Milton calls, this very foolish Rule—this ridiculous provision "D"; because if you recognise the importance of legislating against what would induce you to bend these after punching them, or after drilling them, then all that Mr. Milton has said about provision "D," and all that second paragraph, goes away; it must be entirely swept out. I have only attempted to read this coming up in the cab this morning, and I may not understand what Mr. Milton's meaning is, but I think I am treating it fairly. I need only further remark

this: Mr. Milton says that the condition "V" victimises double-ended boilers to the extent of from 6 to 7 per cent." I do not know whether he chooses the word victimising because it begins with V, perhaps that suggested the word, but I cannot see the appropriateness of it. Now, surely, we all know that the strains upon the circumferential seams upon the bottom of a double-ended boiler are something very different from those upon a single-ended boiler. There is a want of elasticity in a double-ended boiler, especially with through furnaces. If you put through furnaces, perhaps making them in one plate from end to end, and having these furnace plates come within, say, five or six inches of the side of the boiler, then there is an unequal expansion and contraction —a hot furnace and a cold boiler shell—this is one of the grievances, and it is a grievance in this boiler, that, because it had that extra strain put upon it, you have actually given it 6 or 7 per cent. more strength to stand it. He says that victimises it, because you make it stronger when it has more to do. I do not know what the meaning of victimising is, for 6 or 7 per cent. is rather under the extra work it has to do.

The President: I am bound to point out, Mr. MacFarlane Gray, that you have reached the limit of time usually allowed to speakers.

Mr. MacFarlane Gray: My Lord, I could go on in the same way, I think, for every part of this paper.

Sir Edward Reed: I rise to order. This paper involves one of the broadest and severest attacks upon a Public Department that I have known in this Institution. Mr. MacFarlane Gray, it is true, has disclaimed all responsibility for that department, and does not speak in its name; but, as he is an officer in the Department, I venture to suggest whether the meeting would not allow Mr. Gray the necessary scope in point of time for replying pretty fully to the paper.

The President: I quite felt that myself, but still I am here to enforce the rules, and I only called Mr. MacFarlane Gray's attention to the rules. I am quite sure the meeting will allow him that latitude which I am perfectly willing to grant. I am acting simply as your servant in these matters.

Sir Edward Reed: My lord, it has been the usage for the President, when a question of this sort has been raised, to take the sense of the meeting; that is why I rose.

His Lordship having put it to the meeting that Mr. MacFarlane Gray should be allowed to proceed, it was so resolved.

Mr. MacFarlane Gray: Then, my Lord, in the money paragraph—what I may call the "money article" of this paper—Mr. Milton draws our attention to what he makes up to be nearly half a million of money annually which might be saved. Now, there is a lot more money that might be saved; there are a number of millions of bullion which very many, who do not know anything about it, think lie uselessly in the Bank of England when it might be in circulation. But the money is there for a certain purpose; it is for safety that it is there. If this also provides safety, perhaps there is a justification for this being so. Now, let us see whether Mr. Milton is right with regard to these 300,000 tons. In the first place, he has assumed that every owner is anxious to go into 20 per cent. higher pressures. I question that. He has also assumed that every passenger is anxious that the pressure should be increased 20 per cent. in the engine: I question that. And I make this statement, that I think that if to-day the pressure were increased 20 per cent., no more than one-fifth of the number of vessels here would be increased that 20 per cent. of pressure. Now, if we take it as one-fifth, what does it amount to? As it stands, it comes to

something less than 2½d. per head per annum, and that would reduce it to less than a halfpenny per head per annum ; that is to say, that for less than a halfpenny a year, the public can have this security that the boilers that carry them are too strong as pronounced by a rival Institution. Now, is that, or is it not, worth a halfpenny per head per annum ? Well, my Lord, I will not say more, because it really is not fair to the department to which I belong that I should presume to have spoken here this morning, because I believe that those connected with it would be able to make a far better reply than I have made ; but still I think there is something in what I have said.

Sir W. Siemens : My Lord, when gods fall out mortals must suffer, and I present myself to-day here as one of those sufferers, in the character of a shipowner. Nine years ago I had a ship constructed for cable purposes, the *Faraday*. I took great care myself of all the arrangements necessary to make that ship efficient for laying and picking up cables, but as regards boilers and engines I relied entirely upon the Board of Trade Rules and Lloyd's Rules as they then existed, my instruction being simply—" Make the boilers as safe and the engines as efficient as they can be made." The result was undoubtedly a success. That ship never failed to do its arduous duties, being out on the Atlantic sometimes in the winter months for weeks together engaged in very rough work. But in the course of a few years the Board of Trade Rules were altered, and it was intimated that these boilers were really no longer sufficient under the new Rules laid down. We had the boilers carefully inspected after each voyage, and they were pronounced both by the Board of Trade Surveyors and Lloyd's Surveyors to be in perfect condition. Nevertheless, in order to induce us to fall in with the new Rule, the pressure was reduced after each voyage 5 lbs., and we would very soon have been in the happy condition of depending upon our sails. The operations that have to be carried on on board this ship are so important, that I decided to have steel boilers put in, at an expense of £10,000, although the inspection of the boilers proved that they were in perfect condition. Now, this is a hard case to me as a shipowner, because if the two authorities had been agreed regarding the Rules, I should not have been put to this expense, and have had to sell good boilers second-hand, which have gone into other ships not requiring to be under the Rules of the Board of Trade. As an engineer, and one interested in the manufacture of materials, I should say that the Board of Trade Rules give us excessive thicknesses—especially in cases where first-class material is used. I see on page 79 that certain strengths of iron are assumed, not according to the tests this iron can stand, but it is to be taken as equal to 47,000 lb. per square inch in the one direction and 40,000 lbs. in the other. Now this seems to involve a condition of things which does not exist—that of all materials being equally good. I would rather say, have each plate, whether it be steel or iron, tested, and if it does not stand an elongation of 20 per cent. in a bar 8 inches long, it ought to be considered unfit for being put into a boiler. Mr. MacFarlane Gray has given us his reasons why the Board of Trade insist upon a thickness of plate exceeding, as he admits, the necessities of the case. He has given us an instance where, at a pressure of only 17 lbs., a plate had cracked all along the seam, although the boiler was expected to stand 70 lbs. But I fail to follow Mr. MacFarlane Gray in his argument. The forces that come to bear (viz., the strains) upon the material through expansion by heat increase enormously with the resisting power of that material, and therefore the very fact which Mr. MacFarlane Gray has brought before us appears to me to tend rather in the direction of avoiding excess of thickness, because with excess of thickness you get always, whatever be the nature of the material you employ, an inferior plate. But, I say, have sufficient thickness, and be more careful regarding the quality of the material which is employed. If the material of which that boiler was constructed had been such as would elongate 20 or 30 per cent. before breaking, I am

quite certain that the strain put upon it by expansion would have had for its results simply a local elongation, but there would have been accommodation; whereas rigid material has no accommodation, but must yield when, through local action at any one point, the strain put upon it exceeds its absolute strength. This, therefore, appears to me to furnish a very powerful argument in favour of a material of high ductility, and the fault I would find with all these Rules is, that that factor of ductility is not introduced as part of the factor of safety. Now, Sir Joseph Whitworth, who has given much attention to this subject, has laid it down as a rule that the value of a material is composed of the sum of its ultimate strength and of its yielding power under strain. I believe that that is a very excellent rule. Therefore if the factor of safety of 5 is none too much for a material that would break when extended 10 per cent., if it could be made to extend 20 per cent., the factor 5 ought to come down to the factor 4; and you would get with that factor at least as much safety as you would get with the factor 5, and the lower extensibility of the material. This is a question which I hope will be very fully and carefully discussed, and I trust that it will lead to a Rule in which both the Board of Trade and Lloyd's Surveyors concur, in order to prevent the recurrence of such incidents as I have given you.

Mr. MacColl: My Lord and gentlemen, in making a few remarks upon this paper, I shall begin by expressing my personal belief that the effect of the Board of Trade Rules which have been in existence for the last nine or ten years has been to raise the whole tone of boiler-making in this country, and they have done so, not, I think, by improving the better class of boiler-making, but by levelling up, by raising the quality of the work done by those who were previously in the habit of making inferior work. We have all seen boilers made before the existence of these Rules, whose condition was simply absurd, double-riveted longitudinal seams which were no better than single-riveted seams, which were only double-riveted in the sense that they had two rows of rivets, but with regard to strength they were only single-riveted. In making these remarks regarding the effect of the Board of Trade Rules upon boiler-making, I do not lose sight of the fact that a little over eight years since I had the honour of reading a paper before a kindred institution, the Institution of Engineers and Shipbuilders in Scotland, criticising these Rules; and in making the statement I have just made I do not forget that the principles which I then thought were wrong in those Rules I still think to be wrong, notwithstanding what Mr. MacFarlane Gray has told us to-day. I do not believe, and I do not think that engineers in this country believe, that a double butt-strapped and double-riveted transverse seam increases the strength of the boiler over a double-riveted lap seam; and this is proved by the fact that the ideal boiler, as I may call it, is not at present the boiler of this country, nor the boiler that is made to any large extent in this country. I think we shall find that the usual practice in boiler-making is to lap and double-rivet the transverse seams, and to have the longitudinal seams either lapped and treble-riveted or double butt-strapped and double-riveted—unless, perhaps, in a very few exceptional cases, the other system of double-butt strapping and double-riveting the entire boiler has been adopted. Mr. MacFarlane Gray has to-day given us a reason why we should go in for such a very expensive method of making our boilers. But his reason is not one which, I think, we should adopt. I do not think the boiler he spoke of would have been a better boiler, or would have stood better, if it had had double butt-strapped transverse seams. I do not know all the facts of the case, but I should judge that if that boiler had not been rigidly stayed from end to end, and the furnaces had a little elasticity, it would not have fractured at the transverse seam. Upon this point I feel satisfied that the Board of Trade made a great mistake, in insisting upon boilers being made too rigid, and I think over-staying has just the same effect on a boiler that tight-lacing has on a lady. The one is ruinous to the

health of the boiler just as the other is ruinous to the health of the lady. I believe if the Board of Trade would allow us to put fewer stays, particularly at the lower parts, we should make much better boilers than we do. I have no hesitation in saying that. I quite agree with Mr. Milton that most of these additions for different methods of making the transverse seams should be wiped out of the Rules, and that it should be considered that lapped and double-riveted joints in the transverse seams are just as good as anything that can be made. I find, however, that if we take steel—and that is no doubt the material, not of the future, but of the present: I do not know what the practice is in London, but in Liverpool there are hardly such things as iron boilers being made, and the exceptions are only made at the express wish of individuals;—but taking steel boilers, I find that with a thickness less than nine-sixteenths, the Board of Trade, instead of giving us less pressure than Lloyd's, give us more; and if the thickness is between nine-sixteenths and three-fourths, Lloyd's and the Board of Trade practically agree, but if the thickness is over three-fourths, Lloyd's give us an advantage of something like 4 per cent. Now, in view of this I do not see how all Mr. Milton's remarks about the effect of these Rules can be correct. I have not taken the trouble to look into the matter in connection with iron boilers, because I do not, as far as I can help it, have anything to do with them. You will have noticed, no doubt, that the thinner plate gives, according to Lloyd's Rule, less pressure in proportion. That is just quite in opposition to what Dr. Siemens has just told us to be the correct thing. He has told us that the thinner the plate the greater its proportionate strength—at least, I took that to be his meaning; and the sketch made by Mr. MacFarlane Gray is a proof that a thin plate is better than a thick one. Lloyd's have told us to-day why they allow a less proportionate pressure for the thinner plate. From the last paragraph but one, at the bottom of page 78, it appears that boilers are intended to run for four years without a survey. Well, I only express my own opinion, but I think that principle is wrong. No boiler ought to be constructed, and no provision ought to be made for the running of a boiler for four years without a survey. My own opinion is, that every part of the machinery of a ship ought to be surveyed every twelve months—boilers, and everything else. This provision is also made because it is assumed that corrosion is to go on in the boiler. Now, as a matter of fact, the question of corrosion has been thoroughly mastered, at least by some people, I may say; and there are gentlemen in this room to corroborate me when I say that boilers have been built for eleven years, and have been running all that time, and you will not find in the whole of the inside of these boilers one speck of corrosion. Now, if this is possible in one case it is possible in all, and if it is possible in all, why should Lloyd's saddle us with additional thicknesses to provide for something which need not occur? Even if corrosion did occur I do not see that it is right that the original structure of the boiler should be saddled with an increased thickness to provide for something which may afterwards happen. I think if corrosion does go on to a great extent, as is anticipated, the pressure of the boiler ought to be reduced, and that, in order to provide for this, it ought to be the duty not of Lloyd's, but of the ship-owner, to make the provision. Then there is another thing to be considered. If we take a shell only three-eighths of an inch thick, and assume that it loses one-sixteenth of an inch by corrosion, how much does this amount to? One-sixth—17 per cent. of its original thickness; and if it is made with a double butt-strap, there is no reason why its original strength should be reduced one single pound, because the body of the plate only would be reduced in thickness, while the weaker part, the joint, retained its original strength. While the Board of Trade Rules are not all that can be desired, neither is the basis of Lloyd's Rules quite so sound as one would wish. Now as regards the pressure allowed on furnaces. The Board of Trade Rule for furnaces is a very curious thing. There is no doubt about that; still, the effect of both Rules is practically the same, as the compressive strain is limited to 4,000 lbs. to the square inch—or something like that, I am not sure of

the figure, but there is a limit. At any rate, you cannot exceed a certain compressive strain, so that, practically, the thing comes to be about as broad as it is long, taking one Rule with the other. In any case, in making a furnace, a thirty-second or a sixteenth of an inch thicker is not a very serious matter. Then as regards the Rules for flat plates, I shall pass on, leaving that, because I do not think there is much in it. Lloyd's allow nothing extra for steel, but the Board of Trade do; practically, however, for steel, excepting for thick plates, both Rules come to pretty much the same thing. Passing on now to page 83, we find this: "The Rules for shells offer a complete obstacle to further progress in marine engineering, as the thickness of plates now rendered necessary by them to enable existing pressures to be carried, is as great as can be properly closed and riveted together with our present appliances." I have already expressed my own opinion as to these Rules, but if we use steel we can make, by the Board of Trade Rules, a boiler to carry 150 lbs. in a diameter of 10 feet, with a thickness of less than an inch. And no practical man here would say that this is a thickness that could not be dealt with by ordinary machines, and even according to Lloyd's Rules the thickness would only be 4 per cent. less than that. Mr. MacFarlane Gray has touched upon the last part of the paper, and I agree with him in saying that it involves a great fallacy; I mean the loss to the country of half a million per annum. The whole calculation is based upon the assumption that the machinery of existing steamers has been designed for 20 per cent. higher pressure than is at present used, or, in other words, that the pressure has been reduced 20 per cent. I do not think that is the case at all. I think that, as a rule, the pressure which boilers carry is the pressure for which the machinery was designed, and if we increased the boiler pressure 20 per cent., we should find that the machinery was not capable of standing it. It assumes, as Mr. MacFarlane Gray told us, that owners are anxious to increase pressures, and I do not think this is the case. At any rate, I can speak for Liverpool, where the pressure is now about 90 lbs.; we are making boilers to 95 lbs. or 100 lbs., but the general pressure is 90 lbs.

Mr. R. H. Andrews: My Lord, I should like to call attention to the last paragraph of this paper. Mr. Milton there says that he hopes "the President and Council of this Institution may agree with him that this is a subject in regard to which they might with great public advantage bring their influence to bear upon the authorities of the Board of Trade, with a view to having these Rules placed upon a satisfactory basis." I would suggest that if ever the officers of the Board of Trade and Lloyd's Register do put their heads together to try to put these Rules upon a more satisfactory basis, they will advocate the compulsory inspection of all boilers, whether made of iron or steel, during all stages of their construction, by thoroughly competent men, and will also use their influence to induce our legislators to make a law to that effect. The result of that would be this: instead of plates being put into boilers with defects, cracks, &c., in them, which I believe is done, but not intentionally, the defective plates would be condemned by the Inspector, and the boiler made of sound plates; consequently, instead of the Board of Trade insisting upon all these percentages, increasing the thicknesses of plates to make up for the possibility of any defect existing in the plate, as pointed out and illustrated on the blackboard by Mr. MacFarlane Gray—but which perhaps is not there—they would dispense at least with Rules E, J, W, X, and Y. Most of us know that cracks in steel plates cannot always be seen at a glance; they must be looked for, and that very minutely. A part of my duties consists in inspecting steel boilers that have to bear working pressures varying from 120 lbs. to 150 lbs. per square inch, and I find that unless a careful inspection is made of the plates from rivet hole to rivet hole before the boilers are put together, you cannot in many instances discover the cracks that exist. It will not do for an inspector to wait till a boiler is riveted

together and ready for the water pressure test, as many dangerous cracks may not then be discovered, as they may be hidden under laps, or, if exposed, be so fine as to escape detection, and the boiler would be passed as being perfect. The water pressure test may not reveal the cracks, as they do not always weep, although you may have a test pressure of double the working pressure, even 300 lbs. per square inch, but a competent man inspecting the boiler during its construction would object to plates being used that were in any way defective. I just throw out this hint, because I think many people have an idea that all cracks in steel plates are so marked that you can put your hand in them, or, at least, they can be seen at some considerable distance ; whereas I find that, as a rule, they are simply very fine cracks, and necessitate a most careful examination, and perhaps ten to twenty times as long to inspect the boiler during its construction and water pressure test, as it would under an ordinary or casual examination, as adopted in many cases at present. Thorough and constant inspection during construction is, in my opinion, the only way of ensuring reliable work on boilers, and the only satisfactory plan of reconciling the differences that exist between the Board of Trade and Lloyd's Registry as set forth in this paper.

Mr. E. A. Cowper: I think we ought to be grateful to Mr. Milton for bringing this very important subject before us, as it is of very great importance to the mercantile marine that marine boilers should be reasonably constructed. I cannot agree with Mr. MacFarlane Gray that the evils of unequal expansion by heat can be overcome by great thickness of metal. The power of expansion by heat is only limited by the ultimate strength of the metal, and by increasing the thickness and increasing the power the evil effects are in no way done away with, but rather aggravated. When heating up a cold boiler, it is best to blow steam through a moderately-sized pipe down into the bottom of the cold boiler full of water, and thus cause a circulation and an even distribution of heat throughout. There have been many cases of double-flued Cornish and Lancashire boilers leaking through expansion of the flues by heat, whilst the bottom and shell were comparatively cold. I have heard of cases where you could bear your hand on the bottom when steam was up in the boiler. Galloway tubes are very useful in helping circulation. With very long egg-ended boilers, such as are used at iron works, heated from below, the ends curve up considerably on being heated. The Board of Trade Rules made boilers heavier than necessary—I mean unnecessarily strong, particularly for high pressures, and the assumption that the area of the rivets and plates should be equal is not exact, as has been proved by the "Research Committee" of the Institution of Mechanical Engineers. Thick plates, say $1\frac{1}{4}$ inch thick, did not give as good a connection by riveting as when thinner plates were riveted together. Nothing but experiment would give the real strength of thick plates riveted together, or the right proportionate size and pitch of the rivets, for a given thickness of plate ; this had been thoroughly proved by the experiments of the Research Committee, and they had obtained something like 7 per cent. increase of strength. Plates should be proved, and not "taken as of a certain strength," so that a boiler made of superior plates might be made lighter, just as mild steel boilers of proved plates might be made much lighter than if made with ordinary iron plates. It had been said in this room that the Board of Trade Rules were to be taken as correct with the "*usual proportions*" of rivets and pitch, but we were not told what were considered to be the "*usual proportions ;*" now, the "Research Committee" took a great deal of trouble to get at the practice of boiler-makers, and I am sorry to say that it varied so considerably that some were far wrong one way and some another, and there were only a very few near the best proportions of size of rivet to thickness of plate, and pitch of rivets for same. I have such a decided objection to very thick plates, that I would never willingly use a $1\frac{1}{4}$ plate if I could possibly help it. If boilers were proved to double their working pressures,

which was a rule I have settled for myself and always followed, they ought to be well within the "elastic limit" when so proved, so as not to go out of shape or strain at that proof, and there was not much margin wanted beyond that, in addition to something to allow for corrosion. Some time since, through the kindness of Mr. J. T. Smith of Barrow, I had the opportunity of having a small boiler made of mild steel, ¼ of an inch thick, and 4 feet diameter, and having it proved, a lot of men at the pumps pumping up the pressure as fast as possible, when it began to leak at the longitudinal seams and rivets at 420 lbs. per square inch, but it was impossible to burst it, or rend it: it was afterwards caulked up, and stood 350 lbs. pressure. This occurred when I was in the chair of the Mechanical Engineers, and it was the more interesting from the fact of certain steel plates in boilers elsewhere giving way between the time of the making and the proving of this little boiler. Such experiments as this gave actual knowledge of facts, and great confidence in the admirable metal we now had at command. In reference to the amendments necessary in the Board of Trade Rules, it is, of course, only a question of time as to when they could be altered, for a wrong thing could not stand for very long in the face of the wide-spread intelligence of modern engineering; and it depends entirely upon ourselves to move in this matter, and lay before those in authority the very obvious improvements which might be made, and for want of which a trammel, or hindrance, is at present put upon real progress in marine engineering economy.

Mr. William Parker: My Lord and gentlemen, I was very pleased to hear our friend Mr. MacFarlane Gray enter into this discussion, and refer to the boiler which I had the pleasure of inspecting with him. Mr. Gray considers that Mr. Milton's statement that the circumferential seams of a boiler have little or nothing to do with the strength of the boiler is wrong. On that point I differ from Mr. Gray. He endeavours to prove his view by repeating that the boiler of the *Thames*, when getting up steam at a pressure of only 17 lbs., cracked. Well, that is not the only boiler that has cracked in the same way. I think all engineers here will understand and agree with me that if a stupid engineer in charge of a boiler raises steam very rapidly, the top of the boiler must become very much hotter than the bottom; you must naturally expect a great strain, even to the extent of rupture in the bottom plate. There is another case which I now remember, and which very likely my friend Mr. Laird will also remember, where boilers were strained in this manner to the point of rupture; that was the case of a large steamer at Liverpool with boilers of great length, some 18 or 19 feet long. When the steam was being raised in these boilers, the shell plate itself—not the seam, but behind the seam—ripped round for a distance of about 10 or 12 feet. Now this crack was patched, and rigidly patched, and, to the surprise of everyone, the plate on the opposite side gave way in a like manner. I think these boilers were patched on three occasions, when, from a happy thought of some one or another in connection with Mr. Laird's firm, it was suggested that a patch should be put upon each crack, having slightly oval holes, so that when the upper part expanded the lower part would yield to this excessive and irresistible stress. That was done, and the vessel has been sailing without trouble ever since, I think for a period of about ten years. Now, to my mind, that proves that any amount of metal that might have been added would not have overcome this irresistible strain, caused by unequal temperatures, and I think the argument Mr. MacFarlane Gray puts forward altogether falls to the ground. However, the discussion, I fear, has somewhat drifted into a discussion on boiler-making generally, and I would like to lay before this meeting simply and plainly the difference that exists between the Rules of the Board of Trade and those of Lloyd's Register for fixing the safe working pressure on the shells of cylindrical boilers. I have on one occasion before at these meetings had to refer to this matter, and if I use the same argument again, I hope you will excuse me. It is, I think, admitted that if a

decrease of consumption, or more economical steamships are to be obtained, pressures must rise. When mild steel was first introduced for marine boilers—now some five or six years ago—I had the honour to read a paper here on that subject. At that time we had had only one or two boilers made of steel, and the average pressures were then under 75 lbs. Owing to the introduction of steel and other improvements, they have since risen, as Mr. MacColl has told us to-day, to 90 lbs., and I take it that steamship owners would not go on adding weight and increasing pressures unless they obtained an equivalent reduction in fuel. At the present time, owing to the operation of the Rules of the Board of Trade with regard to the shells of these boilers, it is not possible for higher pressures than 90 lbs. to be carried in passenger steamers, unless enormously thick shell plates be used, or the boilers made so small and so numerous that the freight lost by the extra space taken up by the boilers is greater than the money saved by the increase of pressure. Last year we had a paper read upon a steamer with boilers constructed to work at 125 lbs. pressure; that vessel has successfully travelled 43,000 miles without any hitch whatever, and with a considerable saving of fuel. According to the Rules of the Board of Trade, the boilers of this vessel would not be eligible for more than 93 lbs. pressure, but they have been proved by actual work to be quite efficient and perfectly safe at a pressure of 125 pounds. If the Board of Trade Rules were altered to admit of this pressure being carried in boilers of large dimensions, then steamship owners, whose trade compels them to obtain passenger certificates from the Board of Trade, would, like the owner of that vessel, be able to save considerably in the consumption of fuel. The point at issue is of the simplest character. We are now using steel. We test every plate, and we have drawn so many seams asunder, as Mr. Cowper has told us, that we now know pretty well the percentage of strength of any form of joint as compared with the strength of the solid plate. I take it, then, that there is not a very large factor of safety required for either possible inequalities in the strength of the material or uncertainty as to the strength of the joint. Again, we know that the strength of a cylinder varies inversely as its diameter in inches; it is not as if it were a rectangular boiler, or an old wagon-shaped boiler, such as we had years ago, and for which we required a high factor of safety; but here we have a structure with regard to which we know how its strength varies, and I submit that to cover any other contingencies, we do not require such large factors of safety for thick plates as the Board of Trade insist upon. The only other thing for which a factor of safety is necessary is corrosion. Now, it is quite clear that the amount of corrosion is the same in a thick plate as in a thin one. We will just take the case of two boilers, one with shell plates $\frac{3}{8}$ of an inch, and the other with shell plates of 1 inch thick, and assume that the corrosion is an eighth of an inch in each case, then it follows the $\frac{3}{8}$ inch boiler will be reduced one-third, while the inch boiler will be reduced only one-eighth. Now, the difference between the Board of Trade Rules and those of Lloyd's Register is this: we give to the thick boiler at the beginning of its life a larger pressure, or, in other words, approve of a less factor of safety than to the thin boiler, so that at the end of the lifetime of both boilers the thick boiler may have the same factor of safety as the thin one. I think that is all I have to say, except that I hope the reading of this paper and the discussion which has followed upon it may do something towards bringing about further improvements in marine engineering consistently with perfect safety.

Sir Frederick Bramwell: My Lord, I should like to say a few words upon this paper. The question of an official inspection of boilers is, of course, a very wide one. It involves considerations of principle and considerations of detail. I presume at the present time the public would not be content that there should be no official inspection of marine boilers, although, fortunately, there is not one of land boilers. I say fortunately, and do so for the reasons I gave about twelve years ago, when, as I think, a very unwise attempt was being made to put every boiler of every

kind under an inspection which, although it was called an inspection by a steam users' association, would in truth have ended in an official inspection. I think it was about twelve years ago that this was attempted, and the mechanical engineers of London, some twenty of them, got together, and gave such evidence before the committee as put a stop to that which, in my opinion, would have been most mischievous. I know it is very easy, when a man cannot disprove a statement in any way, for him to say, "It is based on figures;" then there is a laugh, and the thing passes over; but I brought before the committee then a statement which, I think, had some weight with them, and that was this: I showed them there that the Rules would apply to boilers working that most wasteful kind of engine used on land, the low-pressure non-condensing engine, and that the result would inevitably be that the inspector in the district, as he would be desirous, of course, of having no explosions in that district, with the object of preventing that, would insist that many of the boilers should be discarded or their working pressure lowered. Owners, if of not much capital, would not like to discard their boilers, and therefore would lower the pressure. A computation was made, to show that probably the result would be to increase the consumption of coal throughout England by four million tons a year. And then this was put to the committee: the statistics of coal mining prove that, taking one year with another over a lengthened period, there is in round numbers one fatal accident for every 100,000 tons of coal raised. The result, therefore, of an increased consumption of four million tons per annum would have been not only a waste of four million tons of our store of coal, but would have been the loss of forty lives of persons engaged in coal mining. The statistics brought forward by those who were advocating the inspection showed that the total loss of lives from all explosions of boilers was, I think, eighty. When we came to analyse that, it turned out that there were a certain number of these due to the bursting of kitchen range boilers and of boilers of that kind, which, of course, could not be touched by any inspection. There were a great number also due to avowed negligence by attendants on boilers, which could not be touched by inspection of the boiler itself; and when such explosions as these were eliminated from the total number, I am speaking now from memory, but my impression is, it brought down the number of deaths due to explosions from preventable causes, and that might possibly have been avoided by care in the making of boilers, to something like fifty-five. It further appeared that the result of inspection was by no means the obtaining absolute immunity from accident, and that thus only forty-six lives instead of the fifty-five would be saved under universal compulsory inspection, while the increased consumption of coal would have cost a loss of the lives of forty miners. Under these circumstances, I ventured to tell the committee that anything might be bought too dear, that you might buy gold too dear, and I said you might buy human life too dear when you bought it with other human life. I am glad to say the committee rejected the attempts of those who were seeking to force this upon us, and left the matter where it was before, viz., that a man should suffer for his own misconduct, if he was guilty of misconduct, in the construction of boilers. That which I have been saying is somewhat, I must confess, beside the particular question before the Institution, although it touches upon the general question. But it does touch even upon the particular question in this way: the inevitable tendency of any department which has responsibility cast upon it is to take care that whatever happens there shall be no discredit cast upon that department, and this drives them to over-caution, involving needless expenditure, and rendering progress and improvement difficult. I remember at that time it was asked of me, in cross-examination by certain members of the committee, "What do you say to the exercise of Board of Trade influence upon marine engineering?" I said that if it had not been for the exercise of Board of Trade influence upon marine engineering, I believed marine engineering would have improved very much

more than it had up to that time. At the date of this committee, marine engines were working at a pressure of about 30 lbs. to the inch. Turning to the Board of Trade Rules themselves, it has been said, and with great force, as it seems to me, that these err on the side of excessive precaution. They begin with a factor of safety of 5; I always thought that the factor of safety was for the purpose of making allowance for those unforeseen matters which might render your boiler less safe than you thought it was. But when one reads through these Regulations, one finds that every attempt is made to get rid of all these chances of uncertainty. For instance, in the factor of safety of 5, nothing is allowed for inferior plate, for although a by no means high quality is assumed in the formula, yet if the Inspector is not satisfied, if he at all doubts the material, there is an addition to the divisor: "X. ·4 to be added when the iron is in any way doubtful, and the Surveyor is not satisfied that it is of the best quality." That is an addition to the factor, taking, therefore, about 1-12th off the pressure. Then, if the boiler has not been open to inspection during the whole period of its construction, 1·65 is added. Now, under this head, you absolutely eliminate all doubt as to the character and quality of the manufacture, because it has been open to inspection the whole while; you have previously eliminated all doubt as to quality of the iron, and having done that, you still have a factor of safety of 5, whereas if you do not eliminate those two things, you are to have a factor of 7·05. Well, is that really reasonable? Where everything is known, is not all that is really wanted a fair allowance for possible wear before a re-survey, when the pressure to be carried may be re-adjusted, and the certainty that the boiler will not be strained beyond its elastic limits. Now, although the whole of these Rules are directed to getting rid of anything in the nature of uncertainty, there is still imposed as a minimum a factor of safety of 5. I cannot help thinking that that is not a reasonable condition, and I do believe that it presses very heavily upon improvements in marine engineering, and upon our power of competing with other nations who are not weighed down to the extent that we are. Everyone must agree that the officers of the Board of Trade desire most rightly and honestly to do the duty cast upon them, and I hope to be understood as only calling attention to the fact that when a man is in a position of responsibility in respect of boilers, the first thing that man takes care of (and it is only human nature), "Whatever else happen, I must be able to report that no explosions have taken place in any boiler I have surveyed or passed."*

Mr. MacFarlane Gray: My Lord, may I say one word of explanation? Dr. Siemens, and Mr. Parker also, attempted to wipe out my remarks about the ripping of a transverse seam. They seem to have misunderstood me, and to think that I recommended (although it was not I at all) thicker plates to prevent the seam ripping. But what I said was this: granting that that would not prevent the seam ripping when the seam has ripped, what is keeping the boiler together? It is the remaining seam that is keeping the boiler together, and if additional strength is not in it—not in

* [The following letter has since been received from Sir Frederick Bramwell.—Ed.]

37, *Great George-street, Westminster, S.W., March* 22, 1883.

Dear Sir,—I had no sooner left the meeting, than I found, to my regret, I had omitted a very striking illustration of the over-stringency of the Board of Trade Rules, viz., that if they were applied to the boilers of locomotives, they would reduce the ordinary working pressure of 140 pounds to so low a point, that the whole railway service, both passenger and goods, would be paralysed. It is some time since I made the calculation, but my recollection is, that the application of the Board of Trade Rules to a new boiler, designed by one of our best locomotive engineers, would have resulted in a working pressure of only 80 pounds instead of 140 pounds. In the interests of speed, and of the power of making full use of our railways and of their plant, and in the interests of economy of fuel, let us hope that the locomotive engineer may for ever be free from Board of Trade trammels.—Believe me to be yours very truly, Frederick Bramwell.

George Holmes, Esq., Secretary, Institution of Naval Architects, 5, Adelphi-terrace, W.C.

the piece that is ripped, of course that is of no value; but when that plate is ripped it is the remaining part, and if that is not of unusual strength it cannot stand the increased pull that comes upon it.

Mr. J. Wright: My Lord, I would like to trouble you with a very few remarks with reference to one part of the subject under discussion, viz., the strength of circular boiler shells and the factor of safety. I think that hitherto we have been over-cautious in this matter, and have looked too much at the wrong end of the subject. Mr. Parker has indicated in his remarks the view which is taken of the subject in carrying out the practice at Lloyd's, and this certainly appears the reasonable view to take. What we should determine is what would be a reasonable factor of safety, or margin of safety, when a boiler would be considered worn-out, and then add to this a proper amount for wear and tear, having regard to the conditions of service and the time the boiler would be expected to last. A fixed factor of safety for a new boiler shell may be unobjectionable for a definite pressure or for pressures not differing much, but it is certainly unnecessary to apply the factor of safety which was used for the steam pressure of a few years ago, say 75 lbs., to the pressures now in use and coming into use, say 100 to 150 lbs., while the conditions of wear and tear, &c., would be very little altered. The *margin* of safety is really what should be taken for guidance, but this, with the usual factor of safety, becomes very much more than appears to be necessary under any conditions at the higher pressures. I may mention that the practice carried out at the Admiralty for some time has been, in the main, on the same lines as Lloyd's Rules.

Sir Edward Reed: May I make an observation or two? I rose at the same moment with Mr. Wright to make the remark that he has made, and therefore I will not repeat that; but I have watched this paper and the discussion rather from a Parliamentary point of view, because it seemed to me that if there is solid justification for the very broad statements made in this paper as to the effects of Board of Trade inspection, Parliament is the only place where effect could be given to the opinions which have been brought forward, and I had some idea of putting a question or two in the House of Commons to the President of the Board of Trade upon this subject. The effect the whole discussion has had upon my mind is this: that the Board of Trade Rules, however meritorious they may have been in the past, are operating perniciously now in two respects, both of which have been spoken to. One has been spoken to by Dr. Siemens and the other by Sir Frederick Bramwell. The first is this: they seem to me to operate against that most important feature in the progress of boiler construction, as in all other constructions in metals, that of thinning the material in proportion as you improve its quality. Mr. MacColl interpreted Dr. Siemens as saying that he would thin the plates almost indefinitely, but I do not understand that to be Dr. Siemens' idea. If I understand him aright, he treats a boiler as a water-holder for the mere purpose of keeping the water together while it receives the application of heat from the outside, and it requires to be thick enough to give the strength necessary for that purpose, and no thicker, except for the sake of the factor of safety. I do believe myself, from my limited experience in boilers, that Dr. Siemens is not only right, but emphatically right, and that a very large part of the mischief that there has been in boilers in the past has arisen from the fact that through the very inferiority of material we have been driven to thick plates, which are attended by the disadvantage that Mr. Cowper has pointed out, and many other disadvantages which we are all familiar with. The use of thick plates in boilers has therefore been at once a consequence and a cause of mischief. Therefore it does appear to me that the present system operates either against the introduction of new and improved material, or else against getting the benefit of the improved material when you have introduced it. If that be so, it seems to me it is a very serious question. Then with regard to the other question, the factor of safety. These Rules do seem to me to operate

injuriously. I would illustrate that by reference to another fact which will be familiar to the minds of all those here, that while we are imposing through the action of the State a factor of 5 (increased, as we have seen, in various ways) upon the boiler, we are content with a factor, if I remember rightly, of less than 2 in the case of the capability of the ship herself to withstand the strain thrown upon her in a sea-way. You will remember, no doubt, that Mr. John read a paper at this Institution in which he showed the result of calculations made upon the strengths of ships to withstand strains at sea, and he there brought out what was to my mind a very startling fact, although I was not wholly unprepared for it, viz., that the factor of safety of a ship as regarded the strain in the sea-way, and therefore as regarded the capability of the bow of the ship to carry the boiler at sea, was really extremely low. I am not quite sure whether it was not less than 2. If that be so, to go on imposing a factor of 5 upon a new boiler made of such admirable steel as is now produced does really seem to me to be improper. I am sorry to trouble you with this, but I should like to act wisely in the matter, and if it happen that we are right in supposing that the Board of Trade Rules in maintaining an unnecessarily high factor operate against the introduction of improved material, which is so advantageous in many ways, some steps ought to be taken to get them amended.

Mr. Milton: My Lord and gentlemen, I will not take up more time than is necessary in addressing the meeting, but I think I must make a few remarks upon those made by Mr. MacFarlane Gray. I clearly state in this paper that it is the strength of the boiler to resist internal pressure for which we take the proportions of the longitudinal seams, and that it is with regard to the internal pressure that we may ignore the circumferential seams. Now, Mr. MacFarlane Gray stated very clearly that the result of all experience of engineers is that the circumferential seams are the seams which give trouble in boilers. This is perfectly true. As a particular case he mentioned that of the *Thames*, where the circumferential seams failed; but these seams did not fail through the pressure of the steam, they failed through the differences of temperature and the consequent differences of expansions on the different parts of the boiler. It is this which makes the circumferential seams leak and give trouble, and in some cases breaks them; but to provide against this you must look to the design of the boiler itself. By putting on factors of safety you make plates thicker, but you do not alter the temperature in the least. You increase the strains in precisely the same proportion as you increase the thickness of the plate to withstand those strains, so that the material is just as much strained with thick as with thin plates.

Mr. MacFarlane Gray: Will you please answer my point of explanation given just now?

Mr. Milton: The design of these circumferential seams does not influence the safety of the boiler at all; the pressure on the ends after the plate is torn is still taken by the stays, which are there to take the pressure before the plate is torn. And if after the seam is torn you can only keep it tight it will never open any further. That has been shown by the boilers that Mr. Parker has referred to. I think the case he must have had in his mind was that of the steamer *Egypt*, which I think had several of her boilers torn in the bottom. They were patched at last with a flexible joint; that is to say, with a joint which gave no longitudinal strength whatever, relying entirely upon the strength of the stays to take up the longitudinal pressure, and those boilers have done good work ever since.

Mr. MacFarlane Gray: They were not cracked. I had the supposed cracks chipped across, and the chipping came off solid with the "crack" mark on it: it was a mistake supposing they were cracked. Their own Inspector could not get in. I got in under the furnaces and had this done.

Mr. MILTON: At any rate, in the case of the *Thames* the plates were cracked. I will mention the case of a steamer belonging to the General Steam Navigation Co. About seven years ago, I saw her boiler, of which the seam was torn for about ten feet—nearly a third round the boiler. That boiler afterwards steamed round to the north with an opening in the plate an eighth of an inch wide made tight by a covering patch, but there was no longitudinal strength added. Seeing that the safety of the boiler is not influenced in any way by the circumferential seams, and that the design of these seams affects only the lifetime of the boiler, or the trouble that will be occasioned in keeping the boiler tight, it should be left entirely to the owners to say whether they will have badly proportioned or well proportioned seams: they will soon find out what are the best. As a matter of fact, all circumferential seams in double-ended boilers are made with double-riveted lap joints, not with double-riveted and double butt-strap joints, to which the Board of Trade allow the smallest factor of safety. After all, the efficiency of these circumferential seams must depend upon the proportion of the diameter and the pitch of the rivets to the thickness of the plate, and that Mr. MacFarlane Gray says must be left to the ordinary practice, which Mr. Cowper has shown varies very much in itself. With regard to the statement of Mr. MacFarlane Gray, that the constants given in the Board of Trade Rules are not accumulative, I have here a paper containing particulars of boilers being made by one of the most eminent engineers in the country, under Lloyd's Rules, for a pressure of 140 lbs. This case will also, in some measure, answer the point raised by Mr. MacColl as to the amount of difference between Lloyd's and the Board of Trade Rules in steel boilers with thick plates. The boilers are passed by Lloyd's Register for 140 lbs. I have seen the tracings of the boilers myself. I am sure they are entirely in accordance with Lloyd's Rules for that pressure. They were submitted to the Board of Trade, who say that 107½ lbs. is the working pressure according to their Rules. This is obtained by adding in the following amounts to the factor 5, for the perfect boiler. First, they add ·3 under clause B, that is, for drilling the holes in the longitudinal seams before the plate is bent: then ·15 under clause G, for drilling holes in the circumferential seams before the plate is bent. Next we have ·1, in accordance with clause S, for the double-riveted lap joint in the circumferential seams; and finally ·3 for the double-ended boiler, as required by the provision V. I may say that these boilers are made with 1⅛ plates to meet our Rules for steel boilers, and I think that is nearly as thick a plate as can well be made of steel. I would not like myself to use thicker plates. If these boilers had to pass the Board of Trade Rules for 140 lbs., the plates would have to be 1 inch and 7½ sixteenths. Mr. MacColl has said that corrosion has now been mastered. Well, it may have been mastered by a few people, but it is not generally mastered. Corrosion, unfortunately, still takes place in very many boilers, and this must be expected when we consider that boilers are sent to sea and are put in charge of men who perhaps have never seen the ship until a few days before she starts—that is an ordinary circumstance of the life of a boiler—and that the engineers are changed continuously. Even if corrosion is mastered by a few engineers we cannot expect that the majority of sea-going engineers have mastered such a difficult subject—a subject studied for many years by the Boiler Committee, and even now not completely mastered by them. Mr. MacColl mentioned also that the Board of Trade give greater pressure for flat plates of steel than for those of iron. When the first steel boilers were made, and they were approved of by Lloyd's Register, it was thought then that steel being a stronger material than iron as regards tension, a smaller thickness of plate could be with safety applied. Several experiments, however, were made (some of them by Mr. Marshall, of Newcastle), which showed that steel was very little, if at all, superior to iron for these purposes in point of strength, especially when you came down to the very mild steel, which is the best for the flat plates of boilers. Lloyd's, in consequence, now give no reduction whatever for flat steel plates

for boilers as compared with iron. This, however, has never worked disadvantageously to the boilers, for the flat plates are not the obstacles to increased pressures. I have nothing more to say, except to thank the meeting for the way in which they have received the paper.

The PRESIDENT: Gentlemen, before I convey, as I am sure I shall be allowed to do, your thanks to Mr. Milton, perhaps you will permit me to say a very few words. You can readily believe, gentlemen, that this is not the first time that this question, of vital importance to the Mercantile Marine, as I believe it to be, has come before my notice. We heard a great deal about it before the Commission on unseaworthy ships, and the great complaint then was that shipowners and shipbuilders were expected to comply with Rules about which they knew nothing, and which they never saw until the ship was either accepted or condemned. That has been corrected; it was a very just complaint. It was the non-circulation of the Board of Trade Rules that was the great cause of complaint. That has been corrected. That shows at least that the Board of Trade is open to conviction. Now let me say one word on behalf of the Board of Trade, because I like to take both sides of the question. We must remember the tremendous responsibility that rests upon the Board of Trade with regard to the thousands of lives of emigrants and other passengers which are committed to their charge annually. The country looks to them to protect those lives, and, therefore, if they err on the side of caution, I am sure you will admit they are rather erring upon the right side than upon the wrong side. I say that for the Board of Trade, and on their behalf. Now we come to another complaint, which is clearly a general complaint, on behalf of the Mercantile Marine, that the Rules of the Board of Trade with regard to boiler plates err in excess of thickness. That point has been put home by Dr. Siemens, by Mr. Cowper, the ex-President of the Mechanical Engineers, by Sir Frederick Bramwell, whose reputation is world-wide, and lastly, but certainly not least, by Mr. Parker, who was himself an officer of the Board of Trade, and who is now the Chief Engineer of Lloyd's. No public department can by possibility afford to neglect such voices as those. I have shown you that they are open to conviction, and when you find between the two controllers of the British Mercantile Navy, viz., the Board of Trade on the one side and Lloyd's on the other—because the term is not too strong—this immense divergence in their Rules in regard to the construction of boilers, I say it is time at least that some attempt at reconciliation between these Rules should be made. And I am sure I know enough of the Board of Trade, and I know them pretty nearly as well as anyone here except the people inside it, to be certain that they cannot afford to neglect the subject-matter of this discussion, and therefore following up my friend, Sir Edward Reed, in Parliament, I would make a very humble suggestion; take it for what it is worth, but I am an old Parliamentary practitioner. I would suggest that we invite the Board of Trade to a conference upon this subject. I think that if a powerful deputation—not too large—of our most eminent engineering members requested an interview with the Board of Trade, it would have a very good practical effect. If you think well of that, I, as your President, not as myself, would be most happy to undertake to make the ordinary arrangements. It must be something more than an ordinary deputation; it must be a conference, because this question affects the whole of the Mercantile Marine of this country, and therefore it is impossible to over-estimate its importance. I hope you will forgive these few observations; but as they are based, to a certain extent, upon Parliamentary procedure and experience, I venture to make them to you. If you agree to them, I will undertake to arrange such a conference, and I am sure the Board of Trade cannot resist the application. They cannot; they would not wish to do so. Now, gentlemen, I am sure you will allow me to convey our united thanks to Mr. Milton for bringing before us this, which, I venture to say, whatever important points we may have brought before us, is as important as any question that can be raised within these walls.

ON SEA-GOING TORPEDO BOATS.

By Mons. J. A. Normand, Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

By sea-going torpedo-boats, we mean boats of from fifty to eighty tons' displacement, having a maximum speed of eighteen to twenty knots, capable of steaming at least 1,000 nautical miles at ten or twelve knots, and of occasionally standing a gale, costing from £8,000 to £11,000, and manned by a crew of from ten to fifteen men.

It seems difficult to overrate the importance of vessels of this class in future warfare. Indeed, should their seaworthiness be proved, the subsequent consequences would follow :—

1st. No iron-clad, no squadron or fleet, no cruisers (unless cruisers should attain the speed of torpedo boats) could navigate in a sea of moderate dimensions, such as the Channel or the Black Sea, belonging simultaneously to Powers at war, unless they should be escorted by sea-going torpedo boats, equal in strength to those of the enemy.

2nd. Military ports situated in those seas or nearer than 200 or 300 miles to the enemy's shores, would be rendered useless as stations for ironclads or cruisers. For instance, supposing a war (which Heaven forbid !) between England and France, this would be the case for Cherbourg, Plymouth, Portsmouth and Sheerness. Cherbourg and Plymouth could then be assimilated to two military ports whose entrances should be under the fire of each other, shot being here replaced by torpedo boats.

3rd. Powers not having military ports sufficiently far from the enemy's shores, should be actually deprived of the use of their navy, with the exception of those vessels stationed in foreign neutral waters, unless they could force the blockade of sea-going torpedo boats with a fleet of the same kind equal in strength.

As the fight between torpedo boats must precede that between ironclads or cruisers and torpedo boats, the former must be armed accordingly with small guns, and especially revolving guns.

It is also possible that they ought to be escorted by one or several transports, carrying as many second-class torpedo-boats as possible, and serving as store-ships. In case of fine weather, the offensive power of the torpedo-boat squadron should be increased, and the reprovisioning effected at sea. The escort would also greatly add to the security of the crews.

The above propositions are founded on the hypothesis that one squadron of sixty to eighty sea-going torpedo-boats, equal in men and cost to one ironclad, are stronger than this ironclad by daylight, and *a fortiori* at night, even when reduced to half its number, the other half having left to coal and reprovision itself.

Could sea-going torpedo-boats be coaled at sea, by means which are yet to be found, or could they, by such means as the use of liquid fuel, have their time of steaming doubled, their importance in warfare would be immensely increased.

No second-class torpedo-boats could replace boats of this kind, because they cannot stand a gale, nor can they be then lowered or shipped, so that the enemy can escape the attack of small torpedo-boats by taking advantage of bad weather.

The question now is, are torpedo-boats of such small displacement as from fifty to eighty tons, really sea-going? If they are not, can they be made so? Time and experience will show, but we already know that with their steel deck and hatchway coverings they can stand very bad weather.

One thing, however, is certain. The use of the torpedo is rapidly gaining a wider range. Before long, we may see it replacing the ordinary ram, that most barbarous of all weapons for ships navigating in squadron, and which has already done more harm to friends than to foes. Should this substitution take place, the ram would remain, but it would be innocuous or offensive, at will. Of the three weapons in use at sea, gun, torpedo, and ram, the first alone can be guarded against by armour, and that, by the sacrifice of money, men, speed, and handiness. As regards the two others, all ships are equally vulnerable. Should torpedo science continue to progress as it has already done, it will be nearly useless to protect ships at such fearful expense against shot, leaving the immersed portion unprotected, and the time may come when fleets will consist of cruisers and torpedo-boats or vessels.

Appendix.

It may be interesting here to note the results of the official trials, made last summer at Cherbourg, of the sea-going torpedo-boat, No. 60, the first of a series built or building by the author, for the French Government.

Hull.

Length at load line	108 ft. 2 in.
Breadth, extreme	10 ,, 10 ,,

Engines.

Diameter, H.P. cylinder	12·60 in.
,, L.P. ,, ...	20·48 ,,
Stroke	14·96 ,,
Heating surface (fire-side)	816 sq. ft.
Grate ,, ...	19·3 ,,

Trials.

1. *Full Speed, three hours' Trial.*

The boat was complete, with launching tubes, compressing engine, air reservoir, six 19 ft. Whitehead torpedoes, 2½ tons coal, two Berthon collapsible boats, no masts.

Displacement	43 tons.
Mean speed	20·62 knots.
Consumption of coal during the three hours	1.58 tons.
,, ,, per hour	0.53 ,,

(We believe this low rate of consumption, taking into account the speed and the weights on board, to be unparalleled.)

Indicated H.P.	about 500.
Revolutions per minute	328·5
Boiler pressure	132 lbs.
Air ,, ...	3⅜ in.

2. *Low Speed Trial.*

Same weights as above, but with five tons more coal, and about half a ton provisions.

Displacement	48½ tons.

The trial lasted 48 hours *without stopping*, from August 19th, 8 o'clock in the morning, to August 21st, same hour.

Weather fine for the first 8 hours, rough for the 40 remaining hours.

Mean speed	12·70 knots.
Total consumption of coal	5·91 tons.

Although the engines raced frequently, all went well throughout the trial.

The boiler was not coated during the trials.

Both trials were preceded by runs on the measured mile.

DISCUSSION.

Mr. Yarrow: My Lord and gentlemen, I am sure I express the feeling of the meeting in saying that we are greatly obliged to M. Normand for laying before us his views on this subject. It shows a very kind feeling on the part of so distinguished a French gentleman to come forward and prepare a paper for an English Institution, as he has done on this occasion, and it would be a good thing to have more interchange of opinion between engineers of different countries, and so long as it is not a one-sided interchange it will be beneficial to both parties. Consequently, in order to reciprocate what M. Normand has initiated, so far as torpedo boats are concerned, I would intimate to the distinguished French gentlemen here that we shall only be too pleased, when

a favourable opportunity occurs, to prepare a paper for some corresponding institution in France. I am almost afraid, gentlemen, that the Boat-race is more interesting than any remarks I would make. Therefore, as regards anything I might say, I would suggest it be sent to the Secretary later on for publication with the Transactions.

The PRESIDENT: That is not in accordance with the Rules, so that anything you have got to say, in spite of the Boat-race, please say it.

Mr. YARROW: M. Normand mentions that the consumption of coal on his full-speed trial is unparalleled, and I take this opportunity of confirming that. I am only sorry that M. Normand is not here, so that we might ask him some questions as to how he arrived at it. As regards sea-going torpedo boats generally, what is meant by that expression, really determines the design of the vessel, and it rests with each individual Government to say what those requirements are. I take it that the two essential conditions of a sea-going torpedo boat are that it shall have a speed, fully equipped, of not less than 19 knots, as anything less than 19 knots under those conditions would, I submit, at the present time not be up to recognised requirements; and, secondly, that a boat of this class should be of as small dimensions as possible, with a given fighting power, that is to say, of two boats having different dimensions, but the same speed and the same fighting power, the smaller one is decidedly the preferable one of the two, because it exposes a smaller area to the shots of the enemy's guns. A sea-going torpedo boat must clearly be capable of going out to sea in any weather. If it is required for the defence of a port and the adjoining coast, I would submit that one of 100 feet long, by about 12 ft. 6 in. beam, gives thorough sea-going qualities, which has been proved by the number of voyages boats of these dimensions have made; they have sufficient coal-carrying capacity and accommodation to conform to the necessary conditions for the defence of a port and adjoining coast, and this may be obtained at a cost of from £8,000 to £9,000. If the sea-going boat is to make long voyages under steam, say 2,000 miles, and with suitable accommodation, I would submit that nothing less than 125 ft. or 130 ft. in length would meet the requirements, and the cost of such a vessel would be about £12,000. Then comes the question, how far it is well for so large and valuable a vessel to be unprotected, and the question at once arises, how far armour protection can be secured, and on what conditions. I believe it is possible to design a vessel 160 ft. in length having all the machinery and torpedo gear encased at the sides with 1½ in. steel, and deck with 1 in. steel, at a cost of about £28,000, having a speed of 19 knots. I introduce the cost because it would seem to me that, having a given sum at their disposal, it rests with naval authorities to say what is the best way of spending that sum. I would say now a few words touching the desirability of having twin screws for torpedo boats. Mr. White, in his excellent work, has clearly pointed out the superior performances of twin over single screws in ordinary vessels, due, no doubt, as he says, to the more advantageous position of the propellers compared with that of the single propeller, as usually adopted. But I do not think, considering the position of the single screw in a torpedo boat, he would expect any advantage in performance by adopting the twin screws. There is one point, which I think an important one, in connection with twin screws for torpedo boats, to which I would draw attention. If it is desired to attack a vessel end on and retire end on, so as to expose as small an area as possible to the enemy's fire, with a single screw vessel, it is well known that, on the boat being stopped and the engines started astern, it will not go back in a direct line, but will turn until sufficient way is secured for the rudders to obtain command, thereby presenting the entire side of the torpedo boat to the enemy's fire, and that at a time when it is almost at a state of rest. If, on the other hand, twin screws are adopted, the boat would be under entire command from the very moment of going astern, and I believe it is the opinion of naval authorities that, if that

could be attained, attacking end on and retiring end on would be a very desirable mode of attack. I would mention that the importance of this point was first pointed out to me by Count Hoyos, of the firm of Messrs. Whitehead & Co.

Mr. J. D'A. SAMUDA: My Lord and gentlemen, it seems to me that we are almost as much indebted to Mr. Yarrow for the explanation which he has given as we are to M. Normand for the paper which he has presented us with, but what I would desire to call attention to is this: that this paper is a much more ambitious paper in its design with reference to the way in which it proposes to use vessels than Mr. Yarrow has treated it as being. From the point of view in which Mr. Yarrow has treated it, that is to say as a torpedo boat, it is very proper to use exactly the arguments that he has used, but what we have to look at with reference to this paper appears to me to be this: Is the author of this paper right in supposing, as this paper evidently does suppose, that by the introduction of such torpedo boats as he describes here, viz., torpedo boats of 50 to 80 tons displacement, we should be able ultimately (to use his own words) to reduce the contest which will have to take place by saying that "the time may come when fleets will consist of cruisers and torpedo boats only." Now I think you must therefore take this paper in the interest of the writer of it with regard to the possibilities of giving effect to his ideas by the means which he puts before us. I venture to think myself that he is entirely wrong in his view. I venture to think that no boat whatever of these sizes would be suitable for the services which he designs to apply it for. I quite agree as to the enormous value that torpedo service is about to render, though probably it has not developed itself sufficiently yet to say it has done that which many of its advocates desire to say for it. But it certainly will go on improving, and will do it. Therefore our right way is to treat it as though it had accomplished the purpose which it is making such very rapid steps at the present moment towards accomplishing. That being so, I agree with so much of this paper as says (and I believe we must all of us agree to this), that it is not desirable to leave our large ironclad vessels which are building at the present moment entirely dependent upon their own means of defence against torpedo boats that may be sent against them. Neither would it be desirable to limit their means of defence to torpedo boats such as they might carry on board their vessels and in their davits. I believe, too, that you will find it an absolute advantage, if not a necessity, to take with the fleet scouts, in the shape of one, or two, or three torpedo boats, accompanying every one of these large ironclads, boats which will be capable of keeping the sea and manœuvring with it. That is what we have to look to; and then the question is, whether boats of this size and description at all approach to the work you require to do. I think not. I think if you would be content to avoid all armour protection, or a very large portion of it, that such vessels, vessels of the sizes that were spoken of yesterday at the time of Captain Noel's paper being read, would be very much nearer the description required. These would be vessels of some 1,200 tons, instead of being 50 to 80 tons' displacement. These would be vessels that would be able to carry large guns; guns which could penetrate small armour-clads, or attack cruisers unarmoured at a considerable distance, and would be a most valuable accompaniment for the fleet, something like cavalry to an army, or skirmishers to an infantry regiment on going into action, or in protecting the ironclad when taking up its position, or in going into a general engagement with the ironclad; but vessels of the small size suggested by M. Normand are wholly inapplicable, and I am not by any means certain, if you could afford the money, that even then you would do well to confine it to such vessels as were spoken of yesterday. I am not quite sure that money should be an absolute *estoppel* to you; and it ought not to be, because in these matters money ought never to be considered. The sole condition which I believe you ought to have in view is to

accomplish your object, no matter at what cost, because I have always held—and I think the thing cannot be too prominently put forward—that to fail in what a nation desires to do in case of war must inevitably entail upon it enormously greater expenses than those which would have been necessary to have enabled it to succeed. I think so, and when we look at what has taken place in modern history—in the result of the late war, for instance, between France and Germany—we see that such great fines are imposed upon the unsuccessful party, that it is quite impossible safely to neglect anything that you can conceive will most effectually carry out the end which you have in view. Going, then, further on in that direction, that is to say, to getting the most complete mode of defending a great ironclad before that ironclad shall be brought into contact with vessels like itself or with any other fleets which it might have to encounter, or with any other vessels which it might have to meet in much greater numbers, but not armoured like itself, even the protection of armour would be of great assistance to these auxiliary vessels that would be accompanying the fleet, and, if you desire to give them a moderate amount of armour, then 1,200 tons would not be sufficient, but you would have in all probability to double the sizes of these vessels. Still, I think myself that it is not at all necessary, in arguing this paper, that one should take a different line of departure from that which is accepted as sufficient by the writer himself, viz., that a vessel of the same description as that which might destroy an ironclad should be the means of opposing and preventing its destruction. That could be very well done without vessels plated with armour, and then I think that with vessels of something like 1,200 tons' displacement you might carry such powerful guns, you might also carry torpedoes, that no torpedo boat could at all approach or come within anything like an attacking distance of your large ironclad, and you would by this means be able to keep off this great attacking force which M. Normand fears might ruin the ironclad and destroy it. As to the idea of being able to have a vessel of 50 tons that should go out and accompany the fleet, it appears to me that such a vessel would be utterly incapable of doing the service that is required of it, that is to say, beyond being a mere torpedo boat itself.

Mr. R. H. Andrews: My Lord, opinions differ as to what is the best size for a torpedo boat. Some men advocate large ones, and some small ones. No doubt both kinds are useful in their way. My opinion is, that small torpedo boats, like the second class in our Navy, are useful under a few conditions only, and principally as picket boats, to give warning of, and keep off boats of the enemy. I have no hesitation in saying, that they can only be used as torpedo boats in fair weather, and that it will be a matter of impossibility to fire the torpedoes from them with any degree of accuracy if there is any sea on. If they were fired, it would be quite as reasonable to expect that they would hit themselves, or their friends, as the enemy. The larger the boats are, the steadier they will be in rough weather, and, consequently, will give a better chance of striking an enemy's ship with their torpedoes than the small boats will. I think there should be large torpedo boats to accompany a fleet, so that torpedoes may be fired against the enemy in bad weather, and also small torpedo boats carried in ships to be used as torpedo boats in fine weather, and as picket boats to keep off night attacks by the enemy's boats in all weathers. I have no doubt that in any future naval war, torpedo boats will principally be used by night, and that it will be desirable to have a large number of these boats in the fleet, so as to form a cordon of one or two lines of boats around your own ships to keep off the attacks of the enemy's boats. I suppose no one will deny that the greater the number of boats, whether torpedo or ordinary boats, the better will be the protection to their ships. I have been told that during the Russo-Turkish war, Admiral Hornby had our fleet anchored in a bay in the Black Sea, the entrance to which was very narrow, and as an experiment he gave orders for a

few ships' boats to go outside the entrance of the bay, and during the night the officers in them were to try to steam the boats into the bay, and up to or near the ships. Sentries were placed on each side of the entrance, the electric light was used from the ships, and picket boats were steaming about, and yet some of these attacking boats passed through the entrance, and got within torpedo attacking distance of some of the ships without being discovered. It seems to me, therefore, that a great number of small torpedo boats will be very useful indeed, and some large ones with a fleet will be indispensable, for the reasons I have given. The question as to the proportion of large to small torpedo boats in a fleet is one which cannot easily be answered, as the cost must be considered as well as other matters. If we suppose a large first-class torpedo boat to cost £10,000, we can get four second-class boats for the same money, but it is very difficult to say definitely which would be the most advantageous way of laying out this money, the purchase of one large boat, or of four small ones. Referring to the results of the full-speed trials, given at the end of the paper, I think that, as Mons. Normand has been so generous in preparing this paper to be read at these meetings, that we might ask him if he will kindly attach to it, to be printed in the Transactions, a drawing of the boiler of the boat in which the trials were made.* My reason for asking this favour is, that the consumption of fuel on the three hours' full-speed trial, is, I think, unprecedented for a torpedo boat; certainly it is in my experience. When we find a torpedo boat of 43 tons displacement being driven over 20 knots per hour on a consumption of fuel of 61·5 lbs. per square foot of grate per hour, or 2·37 lbs. per I.H.P. per hour, we cannot but call it a very economical result, and one that has not as yet been attained in any of Messrs. Thornycroft's or Messrs. Yarrow's boats, under the same conditions. It is practically impossible to make more economical engines than those by these two firms; consequently, the boiler must be a better one for steaming than those usually fitted in our torpedo boats, and therefore a drawing of it would be deeply interesting to us all, and would be a very valuable addition to Mons. Normand's paper, and I trust that he will pardon me for making such a request.

Vice-Admiral De Horsey: My Lord, I wish to add my opinion to that of Mr. Samuda, that a torpedo boat of 50 tons cannot be qualified to keep the sea. We read in the high-speed experiment of M. Normand that she had on board 2½ tons of coal; that she performed her great speed of 20·62 knots very economically, but that during three hours she burnt 1·58 tons. Therefore she could last for something under six hours. Now, I would ask the meeting what use would be a sea-going torpedo boat whose powers were limited to six hours? She might be six hours waiting for the enemy, or sixteen or sixty. I think, my Lord, that the truth of the matter as regards torpedo craft is this, that we must have different sizes for different work. The vessel here described, the 50-ton torpedo boat, would, I have no doubt, be admirable for issuing from our ports to attack an enemy. When you come to Channel work you want a size larger; when you come to Mediterranean work you must have a size yet larger, and when you come to ocean work, there, I think, torpedo *boats* (except those capable of being carried at davits) stop. You may have huge torpedo vessels at enormous expense, and in my opinion there is no reason whatever why we should not build torpedo vessels to go 30 or 40 knots. I believe it is possible; but none exist, and I think this is beyond the scope of the torpedo *boat*. I believe we shall not find it practicable to have attached to the fleet independent sea-going torpedo boats of small size. They will not be able to take care of themselves. If they should expend their coal (and it is reasonable to suppose that that might happen) they would be totally unfit to encounter a gale. Without coal they would lie broadside to the sea; they would be rolled over in all probability by the first very heavy sea that struck them. There was one point to which Mr. Yarrow adverted, as to the advantage of

* See letter from M. Normand, p. 110.—[Ed.]

attacking end on and retiring end on, and he alluded to single screws and twin screws, and said, what is very well known, that with the single screw you cannot go astern without losing your line of direction, and turning to one side or the other. Well, gentlemen, the only plan that I have seen suitable for torpedo boats is that of Mr. John Samuel White's, that of cutting away the dead-wood. You are very likely familiar with that plan, which includes the addition of a balanced rudder *before* the propeller. You can steer the boat going astern just as well as an ordinary boat going ahead. Her powers of turning are marvellous. Dead-wood, as you are all aware, is a contrivance for making sailing vessels weatherly. The moment we come to steamers, dead-wood is immaterial. Those who have not seen experiments with boats without dead-wood would be astonished at their turning powers and steering power going astern, elements which are, above all, requisite for torpedo boats.

Mr. H. Morgan: My Lord, I had had an idea of rising in the early part of this discussion to say that I thought M. Normand was a little too sanguine—I may say a good deal too sanguine—as to the possible power of these torpedo boats, but my friend Mr. Samuda got up and said that as well as I could say it, and I may say he said it so very well that now I am going altogether on the opposite tack. I think, on the other hand, that Mr. Samuda has rather underrated, perhaps, the possible performances of boats of this sort, and he has been followed in that idea by Vice-Admiral de Horsey, both of whom are admirable judges of what might be done. In the outset I would make this little correction. Mr. Samuda and Vice-Admiral de Horsey spoke of boats of 50 tons' displacement. M. Normand speaks of boats of from 50 to 80 tons. We must allow him the credit of his upper limit—80 tons' displacement—which would probably correspond to a boat some 130 feet long. I have had to think a great deal about the possible uses of such boats, and am responsible for having recommended the building of such boats. I am sorry we have not reached the point of having such boats in the English Navy. While, on the other hand, in support of the view I am putting forward, I may quote the fact that such boats are possessed by almost every other country, and I believe I am right in supposing that they are possessed by those countries with the idea that they would do certain work at sea. I may now refer to an event of which we are all very proud, and which has been referred to by your Lordship and other speakers, viz., the bombardment of the forts of Alexandria. If the Egyptians had possessed a few torpedo boats, that bombardment could not have taken place. You may say it could have been prevented by small torpedo boats. So it could by boats like our second-class boats; but suppose the Egyptians had possessed boats of 50 to 80 tons' displacement, which could have followed our fleet some miles to sea, and which need not have cared about encountering some amount of rough weather, and could have remained at sea some time, I am sure in that case our fleet would have found it rather difficult to remain off Alexandria at all. Take another illustration. Suppose that an enemy's fleet were coming across the Channel in tolerably fine weather to attack Portsmouth—or rather, what would first happen, of course, to attack our fleet. If that fleet were accompanied by twenty or thirty or fifty, or you may say one hundred, boats of 50 or 80 tons' displacement, then, I say, the power of that fleet would be most enormously increased against the power of our fleet, supposing our fleet had not those vessels. I am speaking now, of course, of possible occurrences in the English Channel and in moderate weather. I know, as Vice-Admiral de Horsey says, that for different services you want different sizes of boats. We have our little second-class boats, which are carried in ships, and which can be put out whenever there is a chance, whenever the weather and other circumstances, are favourable, which could operate, for instance, in any inland waters, the Black Sea and so on, and which could follow an enemy's ships into their own ports if necessary. Then we have our first-class boats, which are intended to operate from the port—which might defend Portsmouth, Liverpool,

Malta, Gibraltar, or any other such places as those. But I do think if you go a little further and improve the boats in the way they have been improved, in part by Mr. Yarrow and Mr. Thornycroft, that such boats would be found to be very powerful in co-operation with the fleet. Vice-Admiral de Horsey mentioned that these boats could only steam for six hours; but that is at full speed. These boats can go 1,000 knots at half speed. If boats were hovering about the Channel or anywhere else for an enemy's fleet, they could remain a very long time, because they would not want to steam very fast, and they could just go fast enough to take care of themselves in the sea. Vice-Admiral de Horsey has referred to the very excellent plan recently introduced by Mr. John Samuel White. I have the pleasure of knowing something about that plan, having been consulted by Mr. White about it from the beginning, and I have been in several of Mr. White's boats. I will not say any more about it, because Mr. White, while he could not be brought up to the scratch to read a paper here this time, will I hope read one next time. I will only say, in conclusion, that that plan adds very much indeed to the power of torpedo boats, and very likely it will be applied to larger ships as well.

Mr. W. H. White: My Lord, I do not propose entering into discussions on the general policy of naval construction, but I think there are two passages in M. Normand's paper which, in justice to him, ought to be kept in view. I suppose we are all agreed that vessels of various sizes are wanted for different purposes, and Mr. Samuda would not despise these large torpedo boats. If they were in possession of the country there would be a use for them.

Mr. Samuda: Undoubtedly.

Mr. W. H. White: Mr. Samuda's view, as I understand it, is that we should, as a matter of policy in spending, not stand at the money, but build vessels that could do this work and more. So far, I think, there can be no question about it, but M. Normand evidently had chiefly in view the use of this type of vessel within narrow limits of service. He did not quite conceive their going into the middle of the ocean, and waiting for an indefinite time to intercept a fleet belonging to the enemy. His examples are chiefly chosen, you will see, from service in narrow waters and enclosed seas. As a matter of fact (Mr. Yarrow will correct me if I am wrong), I believe that with the rig of which we see an example in the model on the table, these boats are almost self-supporting under sail.

Mr. Yarrow: Yes.

Mr. W. H. White: So that the difficulty which Vice-Admiral de Horsey felt as to the use of coal when waiting is one which could be got over. Another point I should like to refer to is this. These boats are practically unsinkable and uncapsizeable, and the variation in stability produced by burning coal to which Vice-Admiral de Horsey referred might, of course, be got over, supposing them to be capable of proceeding under sail, by water ballast. These are points of detail, but they must be considered in order to do justice, as I know we shall all wish to do, to M. Normand's advocacy of this particular type as adapted for certain uses. The second matter to which I should like to draw attention is at the bottom of the first page in this paper. No allusion has been made to it, but I think it is of the highest importance. M. Normand says, in the last sentence, that the torpedo boat "must be armed with small guns, and especially revolving guns." I think that is the key of the whole position. These boats may be successful against large ships. That is conceivable. They have not been so yet when they have been tried, but they may get a lucky slant and succeed; but they will be kept off, if they are kept off at all, by similar boats; that is to say, a torpedo boat ought to have a powerful machine-gun armament to meet other torpedo boats. To fire a Whitehead torpedo at a rival torpedo boat would be rather a risky and certainly an expensive way of destroying a troublesome foe. If M. Normand were here, I feel confident he would be very happy to give the

information justifying his claim to economy of coal consumption. The figures are, as Mr. Yarrow says, very striking, and I think when he knows—I will not say the doubt, but the hesitation that has been felt about them—he will be very happy to try and justify them as they stand.

The PRESIDENT: Gentlemen, it only remains, as nobody else rises, for me on your behalf to convey to M. Normand our thanks for his paper, and our great regret at his absence and his consequent inability to be here to explain his paper. You observe, gentlemen, the enormous value of your Institution in reference to this and similar papers furnished to us by our foreign members as a means of intercommunication of ideas, not only here in England but in all foreign countries. I do not think we can appreciate too highly the value of that intercommunication of ideas. They have a good deal to tell us; we have possibly a good deal to tell them. Were it not for this Institution, I do not know what means there would be of that ready intercommunication of ideas, without rivalry, and in tones of perfect friendship, merely carried on in the interests of science and progress in shipbuilding. I think you will agree with me that you should look at this question as highly in that light as I do. I am sure you will allow me, gentlemen, to convey to M. Normand our thanks for his very admirable paper.

The following letter relating to the discussion on his paper has been received from Mons. Normand.—[ED.]

HAVRE, *June* 5, 1883.

DEAR SIR,—In answer to your letter of the 1st inst., I regret that I do not feel at liberty, without the sanction of the French Government, to publish the drawings of the boiler of No. 60 Torpedo Boat.

I have heard from friends of mine that the accuracy of the economical performance of the boat was challenged at the meeting, which I regret exceedingly having been unable to attend. The data given in my paper were taken from the *official* report, and Messrs. Thornycroft and Yarrow know themselves with what care these reports are drawn.

I may add that the relatively very small consumption of coals in my torpedo boats is not, in my opinion, exclusively due to the boilers.

I remain, dear Sir, yours very sincerely,

(Signed) J. A. NORMAND.

GEORGE HOLMES, Esq., Secretary to the Institution of Naval Architects, London.

SOME EXPERIMENTS TO TEST THE RESISTANCE OF A FIRST-CLASS TORPEDO-BOAT.

By A. F. Yarrow, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The object of the present paper is to give a brief description of some experiments which we recently tried, to ascertain in a first-class torpedo-boat, propelled at various speeds, the indicated power of the engines, and the thrust on the shaft when steaming in the usual way; also the power required to tow the boat.

The boat upon which the experiments were tried was 100 ft. in length, by 12 ft. 6 in. beam, having a displacement of 40 tons, and for the purpose of towing it at as high a speed as possible we used another torpedo-boat somewhat larger, of about 50 tons' displacement.

The first set of experiments were to ascertain the indicated power for various speeds. The indicators used were of Mr. Darke's latest pattern. The engines were of the ordinary direct-acting inverted surface-condensing type, having cylinders 12½ in. and 21½ in. by 16 in. stroke, capable of making, at 120 pounds per square inch, 480 revolutions per minute, giving a speed of twenty-two and a half knots.

The curve obtained is shown by the line "1" Diagram A (Plate III.), which has been arrived at by numerous observations.

How far diagrams obtained in the usual way with exceptionally high-speed engines may be relied on to represent exactly what passes in the cylinders is no doubt a matter for consideration.

The next set of experiments was to ascertain the thrust on the shaft at various speeds. For this purpose we designed a dynamometer, which is simply a modification of Mr. Duckham's weighing machine. Its exact nature will be clear from Diagrams B (Plate III.) There are two hydraulic rams of a diameter giving collectively exactly six square inches. The two ram cylinders were securely bolted to the bed-plate of the engine, one on each side of the aftermost main bearing, and the rams were secured to the thrust block, which is generally bolted down to a bearer in the boat, but which in this instance was left quite

loose, and allowed to move freely in a forward and aft direction. It was prevented from revolving by means of two stays about seven feet long, secured at their upper ends to the deck. It will be clearly seen that, with this arrangement, immediately there is a thrust on the shaft the thrust block would move bodily forward, pressing the rams into the cylinders. Copper pipes were led from these cylinders to pumps and to pressure gauges fixed in the after cabin, where they could be conveniently observed. As the rams were exactly six square inches in area, and as we could read off on the gauges the exact pressure per square inch, it was a very simple matter to arrive at the total amount of the thrust.

We had three pressure gauges, for the sake of comparison, and we used three pumps, two of them being ordinary vertical reciprocating hand pumps, and the other a ram worked by means of a screw. The vertical hand pumps answered the purpose very well for pumping the ram cylinders full of oil in the first instance, but owing to their intermittent action they were unsuitable for use while any records were being taken.

The pump having its ram worked by a screw we found by its steady and uniform motion to answer well, giving us the utmost pressure required without difficulty. On first testing the apparatus we found the pressure gauges vibrated very considerably, too much so for a correct reading to be obtained, which we believe was due to the unequal strains on the shaft during each revolution. To obviate this, a large air vessel was placed on the pipe uniting the rams with the pressure gauges. The greatest thrust on the shaft obtained was 4,080 pounds at 15.735 knots.

The coupling between the thrust shaft and the crank shaft was of the usual type, steel bolts being secured in the one coupling, passing loosely through holes bored out in the other. Consequently, when the thrust came on the shaft and it moved forward, the amount of pressure registered on the rams would be equal to the thrust on the shaft, minus the friction due to these bolts sliding through the holes and the friction of the rams themselves. Then by working the pump and forcing the stern shaft in a sternward direction, the pressure obtained on the rams would be equal to the thrust plus the friction on the bolts and the rams.

We took the mean between these as representing the thrust, and to be as accurate as possible several pairs of these observations were made at each speed, and the mean of them taken as a fair estimate of the actual thrust. The corresponding horse-powers obtained by multiplying the thrust into the speed of the boat divided by 33,000 gives the curve shown by the line "2."

The shaft not being quite horizontal, the thrust obtained in this way clearly does not correctly represent the horizontal pressure. Resolving the inclined thrust in a vertical and horizontal direction, we find the difference between the total pressure in the direction of the shaft and the horizontal pressure, *i.e.*, for purposes of propulsion the loss due to the inclination, as represented by a quarter to three-eighths per cent. only for speeds within the limits under consideration.

We next proceeded to tow the boat upon which these experiments were tried by another boat, as explained. For this purpose we used the same dynamometer, modified to suit, and adopted the same system of obtaining the pull as we had previously in obtaining the thrust, so that any errors due to the dynamometer itself should be common to both sets of experiments. The exact arrangement of the gear is shown by Diagram C (Plate III.) The dynamometer was secured on the deck of the hauled boat, the rope passing direct from it over a pulley at the bow to the towing boat. The towing experiments were limited to 14·97 knots, which was the utmost speed we could maintain continuously

The length of the tow line we used varied, the mean length being 450 feet, which we believed to be sufficient to avoid the sternward column of water produced by the screw of the towing boat from being perceptibly felt by the towed boat.

At first we found great difficulty in getting anything reliable, as the wave due to the towing boat materially influenced the result. To meet this we tried a number of experiments at as nearly as possible the same speed, varying the lengths of tow line, so that in some cases the waves formed should retard, and in others help the boat, taking the mean of them to represent fairly the correct result, which is shown by the line "3".

In these last experiments the screw of the towed boat was removed.

I would mention that, through the courtesy of Mr. Barnaby, some model experiments were carried out by Mr. Froude upon a very similar boat, having the same displacement as the one upon which we made the foregoing trials. These experiments extended from speeds corresponding to 11·7 up to 23·5 knots.

Although we do not presume that our experiments were conducted with the same care and accuracy as done by Mr. Froude, still it is interesting to note that at those speeds common to both Mr. Froude's trials and ours, Mr. Froude's resistances were less than ours by about 3 per cent.

As the form of the stern of these boats, and the position of the propeller with reference to the stern, are elements greatly influencing the results obtained, I thought a model would better explain these than any design, and therefore beg reference to the model on the table for illustrating these points.

DISCUSSION.

Mr. R. E. Froude: My Lord, I should be sorry to let this paper pass without saying a few words in recognition of the extreme importance of these experiments of Mr. Yarrow with reference to the inquiry which the Admiralty are conducting at Torquay under my superintendence. The work which we have there to do is to test the resistances of a variety of models, and from those resistances to calculate as best we can what will be the resistances of the corresponding ships. The laws on which that calculation is based rest on an extremely strong theoretical foundation as regards the main elements of resistance, but there are undoubtedly some elements of resistance as to which we feel some uncertainty, and therefore any experiments on the actual resistances of full-sized ships, even of full-sized torpedo boats, are of extreme value. Such experiments were made, as most of the members of this Institution will remember, on the resistance of the *Greyhound*, and experiments on the model of the *Greyhound* were also made at the establishment at Torquay, and the results yielded by the model calculations and those of the actual ship agreed very well. But the phenomenon of a boat proceeding through the water at such a very high speed compared to its length as a torpedo boat is of an exceptional character. It is a totally different kind of operation to the phenomenon attending the passage of a vessel through the water at the speeds which full-sized ships attain. Therefore, an experiment on the resistance of an actual torpedo boat is of extreme value, and the more so because the results of the experiments which we have made on models of torpedo boats assign resistances to the full-sized torpedo boats which were certainly large compared with the indicated horse-power which is given by trials on the boats themselves. The theoretical horse-power due to the resistance of the boat multiplied into the speed, as calculated from our model experiments, compared to the indicated horse-power on the trial, does not allow margin enough for the inevitable losses in the action of the screw and engines. Therefore it seems either that our resistances, as calculated for the full-sized torpedo boats, must be in excess, or that the indicated horse-power is underrated, which I certainly think is possible, considering the enormous number of revolutions per minute at which the engines run. Mr. Yarrow states that the actual resistance of the torpedo boat that he tried was 3 per cent. greater than the resistance calculated from the model experiment; therefore that sets my mind quite at ease upon that point. I think it is evident that that is an exceedingly good agreement; I should hardly have expected that we should be able to calculate the absolute resistances as correctly as that. I have only just had an opportunity of seeing the paper, and therefore I have not been able to give sufficient consideration to the method of experiments which Mr. Yarrow has adopted to be in a position to criticise it, but as far as I can see it is extremely ingenious and perfectly well adapted to the purpose. I am very glad in particular to see that Mr. Yarrow has "spotted" (if I may use such a word) the difficulty attendant on the waves following the towing boat. It has been proposed by the Admiralty to make some experiments on a torpedo boat of this kind, though we have not had an opportunity of doing it yet, and in considering how the experiments were to be made the very same difficulty had occurred to me, and I intended to meet it in the same way as Mr. Yarrow has done, by trying experiments at the same speeds with various lengths of tow-rope.

Sir Edward Reed: My Lord, I should like to ask Mr. Yarrow what determines the point of connection of the tow-rope with the vessel. We all know that in towing, the manner in which you take hold of the towed ship is of very great importance as regards resistance, and I should like to ascertain how he knows, when taking hold of the vessel, that the vessel would be towed in proper trim and

would offer the normal resistance. I suppose he took steps, perhaps by tentative experiment, to ascertain where to take hold of her, but I should like to know whether he did, and how it was that he fixed upon the spot that he chose for making the attachment, and whether he made any observations for the purpose of showing what trim the vessel was being towed in as compared with the trim and other conditions when she was being steamed with her own power. That is the only point that occurs to me as requiring some additional information, and that only for the purpose of illustrating the more or less nearness of the theoretical with the practical result and the errors that might enter into it. I am not in a position to form any judgment upon the mode of taking the thrust, but I am really unable to see any defect in it. The method of taking the thrust by means of those cylinders seems to me to be perfect. I think we owe very much to Mr. Yarrow for having made these experiments and communicated them, because, as Mr. Froude said, they lie quite outside any former experience. In the experiments with the *Greyhound* the proportion between the size of the ship and its speed was so totally dissimilar to what it is in this case that the production of such information throws altogether a novel light upon the question of resistance. I had rather hoped to hear from Mr. Froude some remarks as to the relation of these experiments to his own: I did not gather that he said much about them, but as his experiments went to much higher speeds than those of Mr. Yarrow in this care, one would like to know how they compare, and whether he found them fit in as Mr. Yarrow supposed they did. It is not clear to me whether Mr. Yarrow had the information about Mr. Froude's trial with sufficient accuracy.

Mr. Yarrow: Oh, yes.

Sir Edward Reed: I understand that point is clear. Then Mr. Froude has no further light to throw upon this important question at the higher rates of speed.

Mr. R. E. Froude: I should like to say one word in explanation. Mr. Yarrow's curves of resistance did not go above a certain speed, therefore we cannot make a comparison between that resistance and the resistances calculated from the model. I do not see how we can make a comparison at any higher speeds. We have the resistances of the model, but we have not the resistance of the ship to correspond.

Mr. W. H. White: My Lord, I have had an opportunity through Mr. Yarrow's kindness of seeing what was much more interesting even than the diagram, namely, the whole of the apparatus itself, mounted and in working order, and I should like to say that having looked through the apparatus carefully, I think so far as the dynamometrical arrangements go they were excellent, and the result of the experiments which were made to eliminate the internal friction and so on may be considered as trustworthy. I think Mr. Froude named all the principal points in which these experiments are of great value. It is the second large scale experiment that has been made, and the only one where dynamometrical comparisons have been possible between the vessel and the model, and although we have every reason to be satisfied with model experiments as guiding us in the largest power required to drive a full-sized ship at a given speed, there is one element on which we want much more extended information, and in which only large scale experiments can be trusted, viz., the ratio of the effective to the indicated horse-power. What Mr. Froude says is most important. I think its value will be appreciated by members when they come to read this paper and refer back to the papers contributed by the late Mr. Froude to our Transactions, on the ratio of effective to indicated horse-power. Here we have a case, as is obvious from a glance at the diagram, where the ratio of effective to indicated horse-power is exceedingly high. And we have more: Mr. Yarrow has given us not merely the tow-rope resistance, as far as he could ascertain

it, of the vessel, but he has also given us a second curve, which really includes the augmentation—a point to which Mr. Froude did not allude, but which was in his mind: that is to say, taking the curve "3" in the diagram, you have the force required to tow the vessel; taking the curve "2," you have the thrust delivered on the screw shaft; the difference between the two shows clearly what the screw is doing in augmenting resistance. And, in that respect, I think, if gentlemen when they have the opportunity will come and look at this model, they will see that Mr. Yarrow took a great deal of pains to make the difference between the curves "2" and "3" as little as possible, and that it has had a very excellent effect, in some of these marvellous results he has attained in the steam trials of torpedo boats. When we compare the ratio of the ordinates of these two curves, remembering that the late Mr. Froude estimated that the difference between the two was 40 per cent. in the *Greyhound*, those two things, set side by side, indicate what a remarkable gain there is, owing, as Mr. Yarrow well said, to the shape of the stern and the position of the screw. I did hope that we should have anticipated Mr. Yarrow—I am speaking now, if I may say so still, as an Admiralty officer—and have towed torpedo-boats up to the maximum speeds at which they could steam, and have obtained the information in that way. I hope that will be done, and then the information which Sir Edward Reed very properly asked for will be available. Of course it is quite evident that there may be a very considerable deviation in the ratio of effective to indicated horse-power, between what you have when you get to the higher parts of the curve and what is indicated within the speeds of these experiments. It was with the greatest difficulty that Mr. Yarrow could be induced to believe that the facts which he has put before us, and which, I think, are so full of value and instruction, would be sufficiently valuable to put into a paper. I can only say that I fancy I have done some little service to the Institution in convincing Mr. Yarrow that his modesty was altogether misplaced.

Mr. C. Hall: My Lord, I should like to ask this question of Mr. Yarrow—whether his experiment was made in a tideway or in absolutely still water, or whether there was any possibility of disturbance from the tideway? This may perhaps have been explained already, but having come in during the reading of the paper I have not heard it all. It is a fact well known to everyone who has made trials on the Thames, or indeed on any stream, that if the current or tideway is ever so small it will always make a considerable difference in the result, inasmuch as you have the tide helping you for a short time, while it retards you in returning for a long time; possibly this may be the explanation of the small difference of 3 per cent. between the results as found on the actual torpedo-boat, and the results from the models at Torquay. As I said before, it may be that my inquiries are unnecessary; all this may be explained in the paper, but as I have not read the paper I cannot tell whether it is there or not. In a trial of this kind it would seem to me to be an important point, and it is worth while therefore to ask the question.

Mr. Yarrow: My Lord, Mr. Froude mentioned that the difference between the net resistance of the boat and the indicated horse-power did not appear to him to leave a sufficient margin. I fully agree with Mr. Froude; they do not apparently leave sufficient margin. All we have done in collecting these particulars of resistances is simply to describe the experiments, and Mr. Froude will notice that I have thrown doubts over the reliability of indicated diagrams taken at these high speeds. One reason why I do so is this: if we were to take a large diagram at a very high speed, say, two or three inches high, we should find the diagram very unreliable. Therefore, I think we may assume that those sources of error, owing to the long stroke of the diagram, would more or less arise in the small diagram, although of course not to the same extent. Therefore, on these points I quite agree with Mr. Froude, and do not believe altogether in diagrams taken at

these very high speeds, but at present it appears we have no other means of dealing with the subject. Another reason is, that if we compare engines with a short stroke, making a great number of revolutions, and apparently using about the same amount of steam, with a long stroke engine, we do not find the diagrams agree. That rather confirms the opinion that diagrams at very excessive speeds are not to be relied upon, and I think, as a rule, those diagrams indicate less than they ought, which would fall in with what Mr. Froude says. As far as the accuracy of these curves is concerned, whether they are accurate or not I cannot say; I have simply described the experiments, but I believe the line showing the thrust is more to be relied upon than any other, because everything went on very straightforwardly with these trials, and the whole affair was very simple, and in repeating the experiments two or three times we did not find the discrepancies that we did with the other trials. I will now allude to what Mr. Hall said about the boats being tried in still water. All these trials were conducted on calm days, but we had the tide to contend with. They were made at Long Reach. To endeavour to obliterate any errors that might arise with regard to the tide being favourable or unfavourable, what we did was this. We started with very low speeds and gradually worked up to very high speeds, and then we reversed the order of things; and on another day we started with a high speed—the same state of tide—and worked down to low speeds: this would tend to avoid errors which might be due to circumstances beyond our control, and which I think will meet the case Mr. Hall puts. Touching Sir Edward Reed's remarks about the position of the point of towing, I do not quite understand why, if we tow either at one point or at another, it should make any but a very trifling difference; consequently we towed from the point which was convenient to us, as shown on Fig. C, Plate III. Of course, whether that is a good point or a reliable point to tow from I do not know. I simply describe the experiments. Sir Edward Reed asked a question touching the trim of the boat. Now the trim of the boat when towed or when propelled in all cases was the same when at rest; the trim was the same prior to being towed at 15 knots or propelled at 15 knots; but I cannot say whether there was any alteration in the inclination of the boat when moving at these speeds. We did not try anything to test that. I think, gentlemen, that is all I have to say, except to thank you for the kind way in which you have received my paper.

The President: Gentlemen, I am sure you will allow me to convey to Mr. Yarrow your united thanks for this very valuable paper. The discussion that has ensued upon it has been, of course, very interesting. I do not know whether many of you caught the expression that fell from my friend Mr. White, which I will mention. He spoke as an Admiralty officer. You will, I know, all join cordially with me in the feeling of regret that it is probably the last time that he will address us in that capacity—I do not mean during this Session but in future Sessions—in the capacity of an Admiralty officer. You will regret with me that the country is sustaining so heavy a loss in that department, and also congratulate cordially my great countryman upon the accession into his establishment of so much talent.

STEAM YACHTS.

By Dixon Kemp, Esq., Associate.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The prejudice which existed against the introduction of steam in the Royal and mercantile navy extended to yachting, and in 1829 the members of the Royal Yacht Squadron—at that date almost the sole arbiters in yachting affairs—decided that no gentleman owning a vessel propelled by steam power could be a member of the club. This rule was levelled against Mr. Thomas Assheton Smith, of Tedworth, an ardent yachtsman, who had already built five large yachts for competitive sailing. However, Mr. Smith was so fascinated with the achievements of steam that in 1828 he resigned his membership of the Royal Yacht Squadron in order to indulge in his desire to possess a steam yacht. He commissioned Mr. Robert Napier, of Glasgow, to build this yacht, which was supplemented at subsequent dates by the following :—

Date.	Yachts.	Tons.	N.H.P.
1829	Menai	400	120
1837	Glowworm...	300	100
1838	Fire King	700	230
1843	No. 1 Fire Queen...	110	30
1844	No. 2 Fire Queen...	230	80
1845	No. 3 Fire Queen*	300	120
1849	Jenny Lind	220	70
1851	Sea Serpent	250	80

All these yachts, except the No. 2 *Fire Queen*, were propelled by paddle-wheels, and No. 3 *Fire Queen* is still in existence, being the Port Admiral's yacht at Portsmouth.

* This yacht, although engined by Mr. Napier, was built by Mr. John Reid, of Glasgow, and draughted by him in accordance with Mr. Smith's instructions.

No. 2 *Fire Queen* was a screw, and her lines and drawings of her engines are given in Bourne's "Treatise on the Screw Propeller," 1852 edition.

No other steam yachts of any considerable size were built until about the year 1856, when, the Royal Yacht Squadron having removed their edict against steam, Mr. C. R. M. Talbot, M.P. (their vice-commodore) converted his topsail-schooner *Capricorn* into a steam yacht. In the following year there were four other steam yachts (ranging from 30 to 240 tons) in existence. In 1864 there were as many as 30 steam yachts afloat, and from that date they multiplied very fast, until in the year 1877 there existed 280 of five tons and upwards. Since that date the numbers have nearly doubled, and the present fleet of British steam yachts is now made up as follows:—

	Number of Yachts.	Tonnage.
5 and under 100 tons	300	8,550
100 ,, 200 ,, ...	48	6,170
200 ,, 300 ,, ...	25	6,270
300 ,, 400 ,, ...	23	7,700
400 tons and upwards	25	12,732
Total	421	41,422

From the foregoing it will be gathered that steam yachting now occupies a very important place in naval architecture, and it is a significant fact that no large sailing yacht of or above 200 tons has been built during the last six years.

The cause of this can be easily understood if we realise that a large number of men take to yachting for some reason other than a love of the art of sailing or of managing a yacht under canvas; in fact, they know nothing of the art, nor seek to learn anything about it. To these, yachting means shifting about from port to port with unerring certainty, and in agreeable weather. This really forms the charm of steam yachting to those who simply enjoy being on the sea for the sake of being there, or take to it to recruit shattered health, as they can shift ports or do a day's cruising in the most pleasant of all weather on the sea—a calm—when the sailing yacht would be lying helpless.

This leads us to consider whether auxiliary steam power would not be sufficient for the purposes of yachting, so that a man might, as occasions permitted, enjoy all the pleasures and fascinations of sailing, and at the same time be able when necessary to make passages in calm weather or against baffling winds. This was the early idea of steam

yachting, although its originator, Mr. Assheton Smith, had no such divided notion about it; but the first large steam yachts, *Capricorn*, *Erminia*, and *Firefly*, which existed before 1860, had nothing but auxiliary power, and could steam no more than about six knots an hour. At that time, however, the great objection to a full-power steamer was that the consumption of coal was very large, and the boiler and engines occupied more space than could be well spared.

At the present time the relative advantages of the full-power steamer and the auxiliary can be summed up as follows: In the auxiliary, the engine and boiler space in a fore-and-aft direction is within a foot or two as great as in a full-power steamer. The first cost is greater, owing to the greater depth of hull and expense of the equipment of masts, rigging, and sails; and the cost of working is very considerably greater, as a full crew of sailors must be carried, besides the engineers and stokers; and the loss of weight due to the consumption of coal may seriously affect the performance of the auxiliary under canvas, and, moreover, if the coal is not used from about the centre of buoyancy, there will be the difficulty of trim to contend with, which, although it may be of little consequence to the full-power steamer, might be a serious matter for the auxiliary if she had to work against a foul wind.

On the other hand, the auxiliary for extended ocean voyages, such as the *Sunbeam*, *Wanderer*, *Lancashire Witch*, and *Marchesa* have made, has many advantages. She may reel off three or four thousand miles upon a stretch under sail,* and thus effect a great saving in coal and frequently make greater speed, should the wind be strong, than could be made under steam. Beyond this, the auxiliary steam-yacht has greater weight (due to her greater under-water depth), in order that she may carry a considerable weight of ballast, and this has the effect of making her a much better sea-boat than a full-powered steamer would be of similar length, but of less breadth and depth.

The advantages to be gained by making use of the trade winds in ocean voyages are clear enough, but for home cruising, or even for cruising in the Mediterranean, the full-power steamer is the more economical, as she will have to do nine miles out of ten under steam, and to carry about a large crew and heavy outfit of sails which are never required would mean an entirely unnecessary expense.

As might be supposed, steam yachts afford great opportunities for naval architects to achieve a comparatively high rate of speed for any given displacement and engine power; but they are not entirely unfettered in this respect, as the sea-going qualities of the yacht

* The *Lancashire Witch* did the distance (4,458 miles) from the Falkland Islands to Natal in 23 days, the biggest run being 295 miles entirely under canvas; and she did the distance from Yokohama (4,400) also in 23 days, her biggest run being again 295 miles in 24 hours; and she did the whole distance from Tahiti to Liverpool (11,030 miles) in 79 days, having covered, owing to head winds, 12,240 miles. The *Sunbeam*, in her voyage round the world, also did many good runs under canvas, having on one occasion logged 299 miles. The *Sunbeam's* best day's steaming was 230 miles, and *Lancashire Witch's* 216.

must always be considered, and this is a matter which very materially influences form. As a rule, a steam-yacht has much about the same relative fineness in the fore-body that a sailing yacht has; but, owing to her lesser proportionate breadth for any given length, the water lines in the fore-body make a less angle, and the same feature is observable in the after-body. The form of midship section, however, varies very considerably from that of the sailing yacht, which usually has a high bilge; whilst the steam yacht, having to provide capacity for carrying a large weight of coal (usually about 20 per cent. of her total weight), engines, and ballast, carries her side much lower down before it begins to turn in to form the bilge —a feature which has a tendency to promote easiness in a seaway.

Allusion has been made to the introduction of steam in yachting so far back as 1828, and at that date it will be remembered the form of hull supposed to be the best adapted for speed was known as the cod's head and mackerel's tail form; in other words, the entrance was made considerably fuller than the run, and the centre of buoyancy was invariably forward of the mid length. However, Mr. Assheton Smith, in designing his steam yachts, made an entirely new departure, as he designed the entrance at least as fine as the run, and his No. 2 *Fire Queen* had an even finer fore and after body than is to be found in any steam yacht of similar size of the present day.* The year following the

* In 1857 there was a controversy as to whether Mr. Assheton Smith or Mr. J. Scott Russell was entitled to the honour of having discovered the value of hollow lines or waved lines, and Mr. Robert Napier was requested to state what he knew of the matter. This he did in a letter as follows:—

"I am certain that hollow lines were a favourite plan of Mr. Smith long before he ever built a steam yacht; for when contracting for the *Menai* steam yacht (in 1829) he told me that he had put a new bow with hollow lines on to his sailing yacht *Menai*, which had increased her speed greatly, and he wished the same kind of bow put into the steamer, but Mr. Wood, the shipbuilder, being opposed to hollow lines, he gave in; but when Mr. Smith decided to build the *Fire King* he resolved that the lines should be hollow, according to his own plan. Everyone condemned the plan, and continued of that opinion till the day of her trial on the Gareloch, when her unexpected great success changed not only the opinions but the practice of all connected with steamers. From that day a rapid change took place in the form of all steamers requiring speed, by giving them hollow instead of round lines. From all that I know of these hollow lines, the *theory* of them may belong to this or that person, *but that the practical* introduction and adaptation of hollow water lines to steamers entirely belong to Mr. Smith."

Sir Roderick Murchison also stated in a letter that Mr. Smith informed him in 1844 that he was led to consider the wave form when he was a boy at Eton, it having then been pointed out in the course of a lecture that a flat stone when sinking in water described a gentle curve; and assuming it took the line of least resistance, he inferred that the entrance of a ship in the water should be in the form of a similar curve. Sir Roderick then went on to say: "Whilst, however, there can be no doubt that Mr. Thomas Assheton Smith marked out this result [*i.e.* the application of hollow lines to the entrance of ships] entirely by his own ingenuity, it is now admitted by men of science that Mr. Scott Russell is the person who, by analysing the nature, forms, and movements of waves, arrived, by philosophical induction, at the correct application of the wave principle to shipbuilding."

It is not contended that Mr. Scott Russell discovered his "wave of translation" until 1834, when he began some resistance experiments, and in the following year he built the *Wave*, which had a wave line bow. However, so far back as 1830, Mr. Wood, of Port Glasgow, who built the *Menai*, constructed a boat for the Paisley and Ardrossan Canal with hollow lines, and he seems to have been so little satisfied with the result that he persuaded Mr. Smith to abandon the idea in the *Menai*. Several other canal boats were also built in Glasgow with hollow lines about 1835. *Vide* "Reminiscences of Thomas Assheton Smith." (J. Murray, London, 1860.)

construction of the *Fire Queen* in 1844, the Royal yacht *Fairy* was constructed for the use of Her Majesty, and her lines resemble those of the *Fire Queen* in a remarkable manner, but the *Fairy* is the fuller of the two in the ends.

	Fairy.	No. 2 *Fire Queen.*
Length on L.W.L.	140·0 ft. *	127 ft. *
Beam, extreme	21·1 ft.	19·6 ft.
Draught of water	5 ft.	6 ft.
Area of midship section	74·5 sq. ft.	76·2 sq. ft.
Displacement	176·5 tons.	143·5 tons.
Speed on trial	13·3 knots.	12·2 knots.
I.H.P.	364	250
Speed co-efficient $\frac{\text{Speed}^3 \times D^{\frac{2}{3}}}{\text{I.H.P.}}$	198	196

* The screw aperture not included.

Each of these two yachts had the midship section placed three or four feet ahead of the mid-length (not including screw aperture), and both were designed with a view of obtaining the very highest speed, and, judged by the displacement speed co-efficient, they certainly compare favourably with yachts of the present day; and, so far as their individual merits go, they appear to have been about equal. There is, however, no doubt that the co-efficient would have been higher had the two boats been driven by engines of the modern type indicating equal horse-power, as in the contrivances then used to give increased rotation to the screw much useful power must have been lost.

As a means of comparing the forms of various yachts of the present day, a curve of sectional areas has been constructed of eight well-known yachts (Plate VI). All the curves are in the same proportion, the sections in each case having been divided by 10, and the quotient was made to represent an ordinate at the station of each section. To compare the relative fulness or fineness of the fore-body, a curve of versed sines in each case is given as a standard, 5 per cent. being added to the fore-length for the fine end of the curve of versed sines. In all cases this curve would be too fine for the after-body, and, as a means of comparison here, a trochoidal curve is used, with a radius in each case equal to $\frac{1}{10}$th the length of the after-body. It might here be said that the curve of displacement of the *Fire Queen* exactly coincides with a curve of versed sines both in fore-body and after-body.

In the case of these yachts, those whose curves most nearly approach the standard curve are *Xantha*, *Oriental*, and *Marchesa*. So far as the fore-body is concerned, the greatest departure from the curve is to be found in the *Jacamar*, yet she, judged by her speed co-efficient, is a remarkably easy vessel to drive.

With regard to the after-body, the greatest departure from the standard curve is to be met with in the *Aries* and *Queen of Palmyra*. The latter vessel has a small mid-ship section and full ends ; indeed she has a remarkable fulness aft ; and this latter fulness was a necessary condition in her design, as her engines and coal bunkers are very far aft—much farther, in fact, than is usual when the driving power is carried aft.

The *Aries* was built by the Barrow Shipbuilding Company, for Sir James Ramsden. Her displacement curve is not of the wave form, and closely resembles the *Queen of Palmyra's ;* and probably if they were carefully tried under the same conditions, there would not be the difference in their speed co-efficients shown in the table.

The *Oriental's* curve for the fore-body, it will be seen, nearly coincides with the curve of versed sines, and her after-body makes an exact trochoid ; the *Fair Geraldine*, on the other hand, shows a considerable departure in both ends, being much fuller than the standard curves. According to the particulars of the trial trips, given me by the builders, the *Fair Geraldine's* as nearly as possible equalled that of *Oriental;* and I do not think any mistake was made about her speed or I.H.P.,* as her builders, Messrs. Ramage & Ferguson, have recently written to me to confirm the particulars of the trial. The trial of the *Oriental* was conducted by Mr. John Inglis, jun., her designer and builder, and, as the trial was a progressive one, there is every reason for supposing that no mistake was made.

The *Xantha* is a vessel designed by Mr. John Harvey and engined by Palmer & Co. She not only has a wave curve of displacement, but has also strongly pronounced wave lines in her bow. Her speed, however, compared with that of *Fair Geraldine*, is nothing remarkable.

The *Chazalie* was built by Messrs. Camper & Nicholson, and the *Queen of Palmyra* by Messrs. Payne, both being engined by Messrs. Day, Summers & Co. They were very carefully tried on the mile on different occasions, and it will be seen that their speed co-efficients are nearly equal, and are very low compared with the other yachts.

It may perhaps be useful to examine these displacement curves and the alleged performances of the different yachts a little more closely. Upon reference to the tables it will be seen that the *Fair Geraldine* is nearly 16 ft. longer than the *Oriental*, and on her trial was 8 tons lighter, whilst the engines of each indicated 330 H.P. It might reasonably

* The speed of *Fair Geraldine* was verified at Nice Regatta, but her I.H.P. was not indicated.

have been supposed that the greater length of *Fair Geraldine*,* the displacement remaining the same, would have admitted of her being driven considerably faster than the *Oriental* with any given H.P., but, as a fact, her speed was only equal; and there can be no doubt that the comparatively better performance of *Oriental* is due to her finer ends, which, in turn, are due to her greater area of midship section.

The *Fair Geraldine*, it will be observed, compared with the *Oriental*, had on her trial a small midship section, and, to obtain the desired displacement, the ends had necessarily to be fuller. Judged by the midship section co-efficient, the *Oriental's* performance on her trial was a much better one than *Fair Geraldine's*; and this feature is what might have been expected, judging by the results obtained by the late Mr. Froude, in experimenting with an increasing area of midship section, the displacement remaining constant. In the case, however, of *Oriental*, the midship section is not only of greater area than *Fair Geraldine's*, but the hull is shorter; and whether any of the good results obtained are due to the longitudinal disposition of her displacement, which disposition would be dependent upon the form of her lines, I am at present unable to say; but in connection with this subject, I may mention that I have for some time past been making arrangements for the Marquis of Exeter, for testing models of most of the steam yachts, particulars of which are given in the Table which follows, together with various other models, in order to test the value of certain forms of lines and dispositions of displacement in their relation to speed. I hope to conclude the experiments during this year, and shall be pleased to communicate the results to this Institution.

The *Marchesa* was designed and built by Messrs. Lobnitz & Co., and her speed co-efficient with 245 I.H.P. appears to be a very remarkable one; as, even allowing for the low speed of 10·5 knots she was driven at, the co-efficient 283 is something very unusual. Were it not for the equally remarkable speed co-efficient of *Jacamar*, one would be apt to attribute *Marchesa's* achievement to the circumstance that her displacement is distributed in a fore-and-aft direction, nearly in accordance with the wave form; but in the face of what *Jacamar* is alleged to have accomplished, there is nothing to justify one stating that there is any value in such distribution. However, with the knowledge of the uncertainty of arriving at the exact or efficient horse-power used on a trial trip, it would be the merest speculation to ascribe the various results obtained on the trials of some of the seven steamers named to any particular cause.

Fortunately, in the case of the *Oriental*, the trial was conducted with unusual care and completeness; and her speed curve is very instructive in throwing doubt on the alleged results of some of the trials of the other yachts. From this curve we learn that at speeds between two and five knots the resistance increases in a less ratio than the square of the speed, and after passing five knots it exceeds that ratio, until at eight knots it equals the

* The displacement of *Fair Geraldine* on her trial was 258 tons, but with her bunkers full it is 315 tons, and draught of water aft, 10 ft. 6 in.; forward, 8 ft. 3 in.

cube of the speed. From here the ratio increases very rapidly, and at nine knots equals the fourth power of the speed. This ratio is steadily maintained to 11·4 knots, the maximum speed at which the yacht was driven. To have obtained a speed of 12·5 knots at least 480 H.P. would have been required; or the same as it is alleged drove *Jacamar* 12·5 knots, that yacht having 60 per cent. greater displacement. It is possible, of course, that the resistance in the case of the *Oriental* would not increase as the fourth power of the speed up to 12·5 knots, but so far as my experience goes I see no reason to expect a diminution in the ratio of the growth of the resistance.

The speed which *Marchesa* is alleged to have obtained with 245 H.P. is 10·5 knots; now this is exactly the speed the *Oriental* obtained with 245 H.P., and it seems incredible, that *Marchesa*, which has 75 per cent. greater displacement, could have equalled *Oriental's* performance. The speed, 8·9 knots, given by Messrs. Day, Summers, & Co., for *Chazalie* is a reasonable one; but I cannot help concluding that some mistake must have been made in the trials in the case of the *Marchesa* and *Jacamar*.

As the builders of these two vessels also built the engines, they had no reason of course to make the horse-power appear higher than it really was, so long as the guaranteed speed was obtained; and it is just possible that sufficient care was not taken in calculating the diagrams or in counting the revolutions.

In the case where the engines are not built by the firm which constructs the yacht, it is possible that more than the stipulated I.H.P. may be indicated at the beginning of a run when the diagrams are taken, and the I.H.P. may not be maintained all through, as there is generally time to bottle up steam whilst making a turn. Under such circumstances, the speed co-efficient of the yacht may appear to be a very poor one, and such trials might well be considered valueless. I regret that the information concerning the trials cannot be regarded as reliable, and I am sure everyone who has to deal with steam yachts will join with me in hoping that builders and engineers will take a little more trouble in the matter.

As a rule, two runs, with and against tide, are usually made at full power, and the same at half-power; but it would be much more useful if a run were carefully made at less than half-power, at two-third power, and full power; particularly so as from the results obtained from such trials the "economical speed" of the yacht could be calculated.

Coal economy in steam yachting is a very important consideration, and often it is hastily thought that by driving a yacht at her greatest speed she will, by covering the required distance in less time, consume less coal, as there will not be the time to burn it. The fallacy of this can readily be shown by a speed and indicated horse-power curve where the coal consumption per horse-power is known. This consumption in the *Oriental* was found to be 2·12 lb. per indicated horse-power per hour, and on her trial trip the speed made with 52 I.H.P was 6·95 knots, with 90 I.H.P. 8·4 knots, and with 330 I.H.P. 11·4 knots. The coal capacity of the *Oriental* is 33 tons; and assume that she had to

steam 3,500 miles, with no possibility of coaling on the passage, if she started at full speed, and maintained it by keeping up 330 I.H.P., she would exhaust her coal in 105 hours (4 days 9 hours), or when she had only steamed 1,200 miles; but if she started at eight knots, on 82 I.H.P., she would be able to steam the whole distance, 3,500 miles, with the 33 tons of coal; but the time consumed would be 438 hours (18 days 6 hours), instead of 307 (12 days 19 hours) if she could have maintained full speed the whole distance; but to have kept up the speed of 11·4 knots for the shorter time she would require 97 tons of coal. The saving of 64 tons of coal, as against thirteen days' instead of eighteen days' steaming, would be considered a sufficient set-off under ordinary circumstances; and frequently a steam yacht, when making long passages, may be so placed that it would be of the most vital importance that she should be run at the most economical speed. A few years ago, a case occurred in the voyage of the *Eothen* from America to England, when she ran short of coals; and, from ignorance of what the yacht would consume if run at a low speed of about six knots, she was driven at the top of her speed (which, however, did not exceed eight knots), with the result that she could not fetch her port, but had to put into a coaling station. In considering this matter it would also be right to note the effect of putting the extra coal on board. In the case of the *Oriental* the necessary bunker space could not be provided for 97 tons of coal, and if it could the extra 64 tons would put her 11 inches deeper in the water.

I am afraid the coal consumption for indicated horse-power per hour as set out in the tables is not quite authentic, and that for *Fair Geraldine*, 1·7 lb. per hour, is apparently too low. The coal consumption of *Marchesa* was not obtained from the engineers or builders, but from the trials made by the Marquis of Ailsa, at sea, when on a voyage to South America, the West Indies, and other places. No diagrams were taken, but he had the coal carefully weighed day by day, and it was proved on making up each day's run that $2\frac{1}{4}$ tons per 24 hours gave 8 knots per hour; $2\frac{3}{4}$ tons, $8\frac{3}{4}$ knots; and $3\frac{1}{4}$ tons, $9\frac{1}{4}$ knots; the increase of consumption being approximately as the cube of the speed.

Comparative Table of Dimensions, &c.

	Fair Geraldine.	*Marchesa.*	*Jacamar.*	*Oriental.*	*Chazalie.*	*Xantha.*	*Queen of Palmyra.*	*Aries.*
HULL.								
Length over all (stem head to taffrail)	153 ft.	150 ft.	172 ft. 6 in.	143 ft.	169 ft.	116 ft.	143 ft.	157 ft.
Length on L.W.L. to inner stern post	136 ft.	127 ft.	155 ft. 6 in.	120 ft. 3½ in.	150 ft. 6 in.	101 ft.	124 ft.	134 ft. 5 in.
Screw aperture, including outer sternpost	4 ft. 9 in.	7 ft.	4 ft. 9 in.	4 ft. 0½ in.	6 ft. 9 in.	4 ft.	4 ft.	4 ft.
Breadth extreme	21 ft. 2 in.	25 ft.	24 ft.	20 ft. 1¼ in.	28 ft. 6 in.	19 ft.	21 ft.	20 ft. 1 in.
Draught of water 10 ft from fore side of stem	7 ft. 6 in.	11 ft. 7 in.	9 ft.	7 ft. 4 in.	9 ft. 6 in.	6 ft. 8 in.	7 ft.	7 ft. 8 in.
Draught of water aft	9 ft. 9 in.	13 ft. 4 in.	11 ft. 6 in.	10 ft. 2 in.	14 ft. 4 in.	9 ft.	9 ft. 6 in.	10 ft. 2 in.
Mean draught of water	9·1 ft.	12·5 ft.	10 ft. 2 in.	8 ft. 8 in.	12·3 ft.	7·7 ft.	8·2 ft.	8 ft. 11 in.
Area of load water-plane	2080 sq. ft.	2360 sq. ft.	2655 sq. ft.	1726 sq. ft.	2908 sq ft.	1520 sq. ft.	1780 sq. ft.	2089 sq. ft.
Area of midship section on trial	105 sq. ft.	176 sq. ft.	168 sq. ft.	126 sq. ft.	184 sq. ft.	98 sq. ft.	95 sq. ft.	105 sq. ft.
On trial displacement	258 tons.	465 tons.	422 tons.	266 tons.	533 tons.	180 tons.	210 tons.	276 tons.
Co-efficient of displacement	·403	·42	·4	·46	·37	·41	·393	·451
Displacement per inch of immersion at L.W.L.	5 tons.	5·8 tons.	6·3 tons.	4·2 tons.	7·6 tons.	3·6 tons.	4 tons.	5 tons.
Area of immersed vertical longitudinal section	1240 sq. ft.	—	1590 sq. ft.	1040 sq. ft.	1856 sq. ft.	810 sq. ft.	994 sq. ft.	1206 sq. ft.
Midship section abaft centre of length of L.W.L. as above	5·8 ft.	2·5 ft.	6·7 ft.	7 ft.	4 ft.	5 ft.	10·5 ft.	9 ft.
Centre of buoyancy ditto	4 ft.	1·5 ft.	0·7 ft.	1·7 ft.	1 ft.	2·5 ft.	6 ft.	3·96 ft.
Centre of buoyancy below L.W.L	3·1 ft.	—	3 ft.	3·2 ft.	3·7 ft.	—	2·6 ft.	2·84 ft.
Centre of lateral resistance abaft centre of length of L.W.L.	3·1 ft.	—	5·5 ft.	4·3 ft.	7 ft.	—	—	5·85 ft.
Centre of effort of sails ditto	—	—	—	—	—	—	—	2·12 ft.
Area of immersed surface	3212 sq. ft.	—	4500 sq. ft.	2755 sq. ft.	4880 sq. ft.	2190 sq. ft.	2800 sq. ft.	3136 sq. ft.
Area of lower sail per square foot of wet surface	1·07	—	—	—	—	—	—	1·13
Metacentre above centre of buoyancy	—	—	—	4·7 ft.	8 ft.	—	—	5·43 ft.
Ballast	30 tons.	70 tons.	—	40 tons.	79 tons.	—	—	12 tons.
Builder's tonnage	—	—	451	240	604	182	280	268
Registered tonnage, gross	189	266	301	167	328	132	142	187
Ditto, net	128	177	193	80	223	85	103	107
ENGINE-ROOM.								
Fore-and-aft engine and boiler space	31 ft.	28 ft. 6 in.	39 ft.	27 ft. 6 in.	26 ft.	—	28 ft. 6 in.	33 ft. 3. in.
Nominal horse-power	54	60	80	50	60	40	45	50
Diameter of cylinders	18 in. & 36 in.	21 in. & 36 in.	23 in. & 42 in.	18 in. & 32 in.	20 in. & 40 in.	24 in. & 24 in.	16 in. & 24 in.	16 in. & 32 in.
Stroke of pistons	22 in.	24 in.	30 in.	24 in.	24 in.	18 in.	24 in.	24 in.
Mean number of revolutions on trial	111	102	95	146	100	—	113	124
Indicated horse-power on trial trip	330	245	494	330	300	240	272	237
Condensing surface	630 sq. ft.	—	1104 sq. ft.	512 sq. ft.	—	—	—	480 sq. ft.
Vacuum	26 in.	—	26 in	28 in.	—	—	—	26 in.
Diameter of boiler	11 ft. 3 in.	—	12 ft 9 in.	10 ft.	—	—	—	10 ft. 3 in.
Length of boiler	10 ft.	—	9 ft. 9 in.	8 ft. 6 in.	—	—	—	8 ft. 9 in.
Heating surface	1041 sq. ft.	—	1126 sq. ft.	696 sq. ft.	--	—	—	725 sq. ft.
Grate surface	66 sq. ft.	—	49 sq. ft.	30 sq. ft.	-	—	--	36 sq. ft.
Working pressure	70 lbs.	70 lbs.	80 lbs.	80 lbs.	60 lbs.	-	60 lbs.	70 lbs.

Comparative Table of Dimensions, &c. (*continued*).

	Fair Geraldine.	*Marchesa.*	*Jacamar.*	*Oriental.*	*Chazalie.*	*Xantha.*	*Queen of Palmyra.*	*Aries.*
ENGINE-ROOM—*continued.*								
Boiler tested to	140 lbs.	—	160 lbs.	160 lbs.	—	—	—	140 lbs.
Coal-bunker capacity	50 tons.	70 tons.	70 tons.	33 tons.	80 tons.	—	30 tons.	60 tons.
Total weight of machinery with steam up	55 tons.	68 tons.	86 tons.	50 tons.	58 tons.	—	48 tons.	50·4 tons.
Diameter of propeller	9 ft.	—	10 ft.	7 ft.	—	9 ft.	—	7 ft. 9 in.
Pitch	11 ft.	—	14·8 ft.	10 ft.	—	—	—	10 ft.
Surface of all blades	16 sq. ft.	—	—	13½ ft.	—	—	—	14·629 ft.
Number of blades	3	—	—	—	—	—	—	4
Ratio of N.H.P. to I.H.P.	5·55	4·1	6·2	6·6	—	6	6	4·74
I.H.P. per ton of displacement	1·024	·53	1·17	1·25	·56	1·33	0·93	·86
Square feet of immersed surface per I.H.P.	10·7	—	9·1	8·34	16	11·6	12·	13·22
Heating surface per I.H.P.	3·47 sq. ft.	—	—	—	—	—	—	3·06
Grate surface per I.H.P.	0·22 sq. ft.	—	0·1	0·091	—	—	—	·152
Speed on trial	11·7 knots.	10·5 knots.	12·5 knots.	11·4 knots.	8·9 knots.	11·75 knots.	10·52 knots.	10·147 knots.
Coal consumption per I.H.P.	1·7 lbs.	1·8 lbs.	2 lbs.	2·13 lbs.	1·8 lbs.	—	1·8 lbs.	1·85 lbs.
Co-efficient for speed $\frac{S^3 \times M}{I.H.P.}$	510	827	672	567	432	661	524	462
Ditto $\frac{S^3 \times D^{\frac{2}{3}}}{I.H.P.}$	186	283	221	185	155	216	150	186·4
Ditto $\frac{I.H.P.}{M.}$	3·14	1·44	2·94	2·6	1·62	2·45	2·37	2·265
Ditto $\frac{I.H.P.}{D^{\frac{2}{3}}}$	8·2	4	8·1	8·	4·5	7·5	6·3	5·6
SPARS.								
Fore mast, deck to hounds	40 ft.	56 ft. 6 in.	48 ft.	36 ft. 6 in.	57 ft. 6 in.	—	—	42 ft. 6 in.
Main mast, deck to hounds	42 ft.	61 ft.	50 ft.	38 ft. 6 in.	63 ft.	—	—	44 ft. 6 in.
Mizen mast, deck to hounds	37 ft.	—	—	—	68 ft.	—	—	38 ft.
Fore topmast, pin to sheave	21 ft. (*c*)	34 ft. 6 in.	30 ft. (*d*)	15 ft. 9 in.	38 ft. 6 in.	—	—	21 ft. 6 in.
Main topmast, pin to sheave hole	23 ft.	40 ft.	30 ft.	16 ft.	41 ft.	—	—	22 ft.
Mizen topmost ditto	21 ft.	—	—	—	43 ft. 6 in.	—	—	21 ft. 4 in.
Bowsprit outside stem	20 ft.	13 ft.	24 ft.	15 ft.	23 ft. 6 in.	—	—	21 ft. 6 in.
Jib boom to shoulder	—	21 ft.	—	—	24 ft.	—	—	—
Fore boom	36 ft. (*d*)	36 ft. 6 in.	—	29 ft.	35 ft. 6 in.	—	—	34 ft.
Main boom	35 ft.	64 ft.	32 ft. (*d*)	32 ft.	36 ft. 6 in.	—	—	32 ft.
Mizen boom	38 ft. 6 in.	—	44 ft.	—	65 ft.	—	—	32 ft.
Fore gaff	20 ft.	27 ft.	—	—	28 ft. 6 in.	—	—	24 ft.
Main gaff	17 ft.	29 ft.	24 ft.	18 ft. 6 in.	29 ft.	—	—	24 ft.
Mizen gaff	20 ft.	—	25 ft.	19 ft. 6 in.	37 ft.	—	—	20 ft.
Squaresail yard	39 ft.	—	48 ft.	—	65 ft.	—	—	—
Square topsail yard	20 ft.	—	—	—	33 ft.	—	—	—
Fore mast, on deck, from a perpendicular to fore side of stem at L.W.L.	28 ft.	40 ft.	32 ft.	26 ft. 6 in.	32 ft.	—	—	28 ft. 9 in.
Main mast from fore mast, centre to centre	42 ft.	39 ft.	69 ft.	56 ft. 6 in.	38 ft.	—	—	45 ft. 9 in.
Mizen mast from main mast	40 ft.	—	—	—	39 ft.	—	—	41 ft. 9 in.

DISCUSSION.

Mr. Martell: My Lord, I think it would be a great pity, after the trouble that has been taken by Mr. Dixon Kemp and the great intelligence displayed by him in producing this paper, that it should pass without a few words of recognition and of praise on the part of this Institution for the trouble he has taken in placing it before us for our information. Irrespective of the letterpress part of the paper, I think this tabulated statement at the end is very interesting, and is well worthy of a place in the records of this Society. Yachting is now becoming a matter of so much interest to the large and wealthy portion of the population of this country, and yachts are so much increasing in number—and steam yachts in particular—that any information we can derive of a reliable character such as this, is very interesting and important, and I can only say that I for myself—and I am sure I can speak on the part of the Institution—return our thanks to Mr. Dixon Kemp for the great care that he has taken in giving us these records, and enabling us to form our opinions when we shall have time to look over these results, as to the merits of the various steam yachts of which he has given us the calculated results in these tables, and for enabling us to place this upon the records of the Institution.

Mr. A. J. Inglis, Jun.: My Lord, I was not aware that Mr. Dixon Kemp intended to adduce the *Oriental* in his paper as an example, or I might have been able to supplement the information he has given with some other data that we have. We do not consider the trials of the *Oriental* by any means complete yet, or that this trial, of which Mr. Dixon Kemp has given us the figures, exhausts the subject. We had some difficulty in arriving at the best form of screw propeller for this vessel. This, of which we have particulars here, is the best. It is the third we have tried, and it is the best as yet, but I do not think that we have yet got to anything like finality in this direction. I think that something more can be got out of the vessel yet by further experiment. The first propeller we had gave a higher maximum speed, but the speed of piston was considered by the owner to be too great for comfort, and he preferred, even if the speed were less, a smaller number of revolutions. We got not quite the same maximum speed, but, on the whole, a rather more efficient performance. The coal consumption, as put down here, is not quite accurate. It is stated in this table to be 2·13 lbs. per indicated horse-power, but in point of fact we never measured the coal consumption at all. As an illustration of the value of progressive trials for arriving at the most economical speed to drive a vessel at, in corresponding with Mr. Dixon Kemp on the subject, I took care to assume a high enough consumption of fuel—one that would be quite safe; and taking this at 2·13 lbs., it came out at even figures in some of the calculations we made, and I thought it was better to err on the safe side. I do not think that the consumption would be anything like as much as 2 lbs., especially if the vessel were driven at eight knots. It is a great pity, as Mr. Dixon Kemp says, that a little care had not been taken at the trials of the other vessels, and that they were not all tried on the progressive system that has been so often advocated here by our friend Mr. Denny, who, I regret, is not present on this occasion. I think that comparisons of the performance of vessels of this kind, in which considerable speed is attained with small dimensions, would be much more interesting if a little more care were taken by builders to get really reliable figures.

Mr. Dixon Kemp: My Lord and gentlemen, I simply have to thank you for the attention that has been paid to my paper, and for the manner in which you have received it.

The President: Gentlemen, I know that I may in turn convey to Mr. Dixon Kemp your best thanks for his valuable paper.

The PRESIDENT: I never like to suggest anything of this kind, but I think it might be well to take the next two papers together, that is to say, to have them read the one after the other. They are very much on analagous subjects, and it might be convenient to read them one after the other, and take the discussion then upon the two papers at the end. Perhaps Mr. Wigham Richardson would not object to that, nor Mr. Smith?

Mr. W. E. SMITH: I have no objection.

Mr. WIGHAM RICHARDSON: Neither have I.

The PRESIDENT: It would certainly be a convenience. I will take the opinion of the meeting.

The proposition was unanimously assented to, and the following two papers were taken together.

ON THE MODES OF ESTIMATING THE STRAINS TO WHICH STEAMERS ARE SUBJECT.

By WIGHAM RICHARDSON, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the EARL OF RAVENSWORTH, President, in the Chair.]

I PURPOSE very briefly to point out the modes which are current of treating the strains affecting steamers and the fixing of the scantlings of their ironwork; and let me here say, for the sake of clear definition, that when in this paper I speak of *length* as affecting girder strains, I mean in all cases the ratio of length, *i.e.*, the length divided by the depth of the girder, or in other words, the *length expressed in units of the depth of the girder.*

Thus, in wracking strains this will be $\frac{\text{length}}{\text{beam}}$, while in hogging and sagging strains it will be $\frac{\text{length}}{\text{depth}}$. This is irrespective of the material composing the girder. In actual calculation the depth is taken to the true centres.

The most obvious way of regarding the ultimate girder stress on a steamer is to suppose her loaded and supported on the crest of a bank, and not in any way water-borne (see Fig. 1, Plate VII.) The stress in this position will manifestly depend upon the weight of the fore and after bodies, respectively, and upon the distance of their respective centres of gravity from the point of suspension.

Thismethod, and the finding of the neutral axis, are very fully gone into in the work on shipbuilding written by the late Sir William Fairbairn; but it is a stress to which it is usually considered a ship ought never to be subjected. It is contended, and perhaps with reason, that a steamer is built to float, and not to be stranded. Still, this way of regarding girder strains has so much to recommend it, that whenever my firm builds a steamer without a classification certificate, we always adopt this method. Such instances have usually been paddle steamers intended to carry little or no cargo beyond bunker coal; but it is worthy of remark that these steamers, which were calculated to bear this stress, and which in more than one instance have borne it successfully, should have been considered inadmissible by the registries. So long as the load-line remains an open question, this is, perhaps, inevitable.

The actual stress upon a vessel at sea is not so easily estimated, but it is a fraction of the stranded strain, and will not vary widely from a constant fraction. It has been variously estimated by different authorities. In Mr. William John's paper (Vol. xv., p. 74, of the Transactions) it is stated that the displacement multiplied by $\frac{1}{35}$ of the length will give a fair average for the maximum bending moment of merchant steamers. This bending moment is, I understand, irrespective of the depth, and therefore a different thing from the hogging stress on the upper works. By the method which I think was first largely gone into at the Admiralty when Sir E. Reed was Chief Constructor, waves are assumed to be of a certain length and height (see Fig. 2, Plate VII.), and the bending moments are then calculated.

It is sometimes argued that very long ships may overlap two wave crests, but as it is believed that some waves are over 1,000 feet from crest to crest, it cannot be safe to count upon this, at least not in steamers designed for general trades.

It is usual to speak of *hogging*, *sagging*, *wracking*, and *torsional* strains, but all of these are derived primarily from the weight of the steamer and of her cargo (in other words, from the displacement) and from the length. It would be foreign to the purpose of this paper to consider minor or local strains ; it is sufficient to show that in fixing the scantlings of the midship section, the principal stress will always depend on the displacement and the length. This, no doubt, will be accepted by all who have paid any attention to the subject.

Let us now consider how these strains are provided for, how the problem is faced.

The Admiralty are believed to calculate the strains on every vessel, and to determine the material accordingly.

Private firms no doubt vary widely in their practice. I have given that of my own firm, and in Vol. xvi., p. 190, of the Transactions you will find a further investigation of what should be allowed if the steamer is water-borne.

The Veritas Society have a table of scantlings for different sizes of vessels, the successive grades of which are determined by the contents of the parallelopiped of the length multiplied by the beam and by the midship depth (see Fig. 3, Plate VII.). A deduction is made of 30 per cent., but of course this has relatively no effect on the formula, for it is evident that if a has a certain relation to b, the same will hold good for fractions of the same, or—

$$\frac{a}{b} = \frac{\frac{7}{10}a}{\frac{7}{10}b}$$

So far as I know, except in extreme proportions the Veritas Society neglects the element

of length into displacement. If, therefore, we could consider that all vessels have the same relative freeboard, we might characterise the Veritas method as discarding all differences of model, but otherwise a rough approach to basing scantlings on an average displacement irrespective (except in extreme proportions) of length.

The Underwriters' Registry takes as a standard the under-deck tonnage, either as measured or as taken from the plans, neglecting also, except in extreme cases, the element of length into displacement. This plan (see Fig. 4, Plate VII.) is therefore a much nearer approach to a displacement basis than the Veritas, because it takes the model into account, and if applied only to cargo vessels loading up to the limits of a well-recognised scale it would be a very close approximation to a displacement basis, but irrespective (except in extreme proportions) of length.

Lloyd's Registry used formerly to vary their grades also with the under-deck register tonnage, but latterly they have adopted instead two sets of numerals, which are arrived at in the following manner :—

Table G contains the grades of numerals which regulate the scantlings of the frames, reverse bars, &c. Each numeral is the midship depth of the steamer in feet measured from keel to deck, plus the half moulded beam, plus the half girth ; that is to say, it is the length of the black line in Fig. 5 (Plate VII.).

This is as far removed from a displacement basis as it is well possible to conceive. If we assume two exactly similar vessels, the one twice the dimensions of the other, the displacement (if the draught of water be also doubled) would be increased as the cube of two (2^3), that is, eight times, whereas the numeral in Table G would only be doubled.

The plating, the stringers, &c., are regulated by Table G2, the numerals of which are the numerals of Table G multiplied by the length of the vessel. This gives the prismatic surface of the midship section as taken in Fig. 5, prolonged to the ends of the ship (see Fig. 6, Plate VII.). This also is far removed from a displacement basis. Except to the small extent of the rounding of the bilge, on one side only, the difference of model is not taken into account at all, and for progressive sizes in similar vessels the scantlings in the table would increase as the squares only, while the displacement and the strains increase as the cubes. Thus in the illustration above, where one steamer is twice the dimensions and draught of the other, the displacement would be increased eight times, whereas the numerals would be increased only four times. The effect of this in large or very long steamers is easy to be seen. The weakness of large steamers was well described by Mr. William John in the paper of his before alluded to (see page 80 of Vol. xv. of the Transactions), and on page 111 of Vol. xviii. will be found a woodcut which gave in a graphic form some calculations made by the staff of my firm under the direction of my partner, Mr. Charles Christie. The calculations were from Lloyd's Rules in force in 1877, but although

additional material has since been added, the basis of Tables G and G2 remains the same, and therefore I reproduce the diagram (see Fig. 7, Plate VIII.). The additional material required by the rules at present in force would carry the curve of material higher up, as shown. The curves, which have been derived from actual calculation in each instance, show clearly enough, that while the strains vary as the square of the length, the material to meet them varies more nearly as the first powers. It must be noted that in this particular calculation the midship section is assumed to be the same with each different length, wherefore the strains vary as the squares, not as the cubes, of the lengths.

As regards differences in model, seeing that strains depend so directly upon proportionate dimensions and upon displacement, it will probably be generally conceded that a wide door for error is left open when these are attempted to be *averaged*. The co-efficient of fineness of displacement, reckoned from the top of the keel to the load-line, has been in vessels recently built as high as ·81 or ·82, and I suppose in some vessels of special type it will be found to be below ·50. In Fig. 7, I give the strains relatively with models of ·50 and ·78 of the parallelopiped taken to the bottom of an ordinary keel.

The classification societies, forming their rules tentatively, have preferred, not perhaps unnaturally, to make out tables and prescribe rules, both based largely on practice, rather than to enunciate principles or simply to prescribe a limit, as the Board of Trade do in the case of bridges. It is not therefore easy, or even possible, to say how far any of them recognise the depth of the girder as an element of strength. The calculations shown in Fig. 7 refer to the practice of one Society only. Similarly as regards an added constant for corrosion, it is not stated how much is allowed.

It is probable that changes will ere long be made in the rules of one or all the classification societies. Many shipbuilders are far from satisfied, and it is in every way undesirable that we, the builders, who are the designers and constructors, should not be content with the regulations of those who in so many respects assist us and aid us, and to whose funds we are the principal contributors.

HOGGING AND SAGGING STRAINS IN A SEA WAY AS INFLUENCED BY WAVE STRUCTURE.

By W. E. Smith, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The subject of the longitudinal strength of ships has been so ably and so frequently treated of in this room, that it is with some hesitation I venture to address the meeting on any part of this important matter.

The researches of Sir E. J. Reed, Mr. W. H. White, Mr. John, and others, have created an extensive literature dealing with nearly all kinds of ships, the examples treated of ranging from different types of ironclads to despatch vessels and yachts in the Royal Navy, and from the largest merchant ships to the shallow draught *Mary*, which foundered in the Bay of Biscay under circumstances that received a full discussion at the time.

The rapidly growing numbers of very long ships—ships ranging from 450 to 550 feet in length, and whose scantlings necessarily increase in a more rapid ratio than their dimensions—affords, however, some excuse for drawing renewed attention to the strains brought upon ships by the continually varying buoyancy afforded by a stormy sea.

The general method of making investigations of this kind is so familiar to the members of this Institution as to make it unnecessary to explain it in my paper. Two curves are calculated, one representing the longitudinal distribution of the weight of the ship, the other the distribution of support or buoyancy given by the water. The difference between the two constitutes an excess of load or of buoyancy, and serves as the basis of the numerical investigation.

In the examples hitherto published, the distribution of buoyancy along the length of the ship has been assumed to be the same as the distribution of displacement (*i.e.* of immersed area of transverse section), both for still water and for wave water. It has, however, long been known that the buoyancy of wave water for a given volume of displace-

ment is not the same at all parts of the wave, the buoyancy corresponding to that displacement being less at the crest of the wave and more at the trough of the wave than the weight due to the volume displaced.

The great numerical importance of this feature of wave water was first pointed out some years ago by Mr. W. H. White, and although for some purposes it may be safely neglected, he showed in his paper on the Rolling of Sailing Ships (in preparing which I had the honour to assist him), that for others its practical influence is very large.

I have recently been extending the application of this feature of wave water to the calculation of the longitudinal bending moments in ships in a sea way, so far as they are produced by variations of buoyancy, and the results obtained cannot, I think, be without interest to the meeting.

The method adopted in performing the work was as follows:—A diagram similar to Fig. 4 (Plate IX.) was constructed on a large scale, showing accurately the internal structure of the wave. Each wave line represents a surface of equal pressure, the numerical value of which was calculated. In the diagrams these pressures are marked p_1, p_2, p_3, &c. The ship is then placed in the wave, and the buoyancy at each section is estimated graphically as shown in Fig. 7 (Plate IX.). $W'L'$ is the height of the wave at the section chosen. At the points, 3, 4, 5, &c., on the immersed part of the section the pressures are p_3, p_4, p_5 . . .

Vertical lines of lengths p_3, p_4, p_5 being set up from the points 3, 4, 5 . . . and a fair curve $w'l'$ passed through their tops, we get the amount of buoyancy at the cross section chosen. The section shown in the figure is at the wave crest. The area of the section below $W'L'$ marks the *displacement*, and the area below $w'l'$ marks the *buoyancy* at this section, the shaded part representing the difference between the two. WL is the line at which the ship floats in still water. The excess of buoyancy amidships at the wave crest over the buoyancy in still water is the area between $w'l'$ and WL, and this, as will be seen, is not nearly so much as the area between $w'l'$ and $W'L'$. The converse holds good at the wave trough, the buoyancy there being greater than the weight of water displaced.

The buoyancy at each transverse section was obtained in a similar manner, and a curve of buoyancy drawn for the whole length of the ship. This curve of buoyancy therefore differs from the curve of buoyancy or displacement used in previous investigations, in that it makes the buoyancy at the wave crest less and at the wave trough more. The variation of buoyancy from the still water condition of buoyancy is therefore less, as I have calculated it, than if the buoyancy is supposed proportional to the displacement. The variation of bending moment, taking the buoyancy as influenced by the wave structure, must in like manner also be less than if we take it as being measured by the area displaced at each transverse section.

The object of my paper is to place on record the relation between the bending moments calculated on the two hypotheses.

The difference between the two results depends, as will be seen by the following examples, to some extent upon the form and to a much larger extent upon the draught of water of the ship, and in some cases reaches a considerable amount.

To avoid complication and crowding in the figures accompanying the paper, I have given only the resulting curves of bending moments, the dotted lines showing the moments obtained after allowing for the influence of wave structure, and the solid lines the bending moments not allowing for this influence. In all the examples given the length of ship is 315 feet, and the height of wave 20 feet.

Fig. 5 (Plate IX.) gives the results for a vessel of very fine form, both as regards water lines and also as regards rise of floor. To ensure great manœuvring power the keel is cut up very much at each end of the ship, so that the buoyancy is very much concentrated amidships. The mean draught is 18 feet.

In this example, calling the maximum hogging moment + the maximum sagging moment 100 as calculated by one method, it is 145, as calculated by the other—nearly half as much again.

The difference between the maximum hogging moments obtained by the two methods is not very great; but the sagging moment, not allowing for wave structure, is practically double that obtained after making due allowance for wave structure.

I need not point out how important this difference is, as regards the tendency to produce buckling in the upper works of the ship.

In this example, the actual distribution of the weight of the ship has been taken as the basis of the numerical work. A different distribution of weight would alter the proportion in which the hogging moments and the sagging moments were separately affected, but would still leave the same difference of 45 per cent. between the sum of the two.

For the purpose of investigating the influence of form of vessel in affecting the difference between the two modes of calculating, I have, therefore, not worked out the actual distribution of weights, as this difference of maximum hogging + maximum sagging will be independent of it. One distribution of weight would throw more difference between the hogging moments, while another would throw more between the sagging moments, calculated by the two methods.

Fig. 6 (Plate IX.) gives the still water distribution of buoyancy of three ships, all of which are of very full section, and all have a mean draught of 25 ft. 9 in. in still water. I have not drawn the complete curves of hogging and sagging moments, but the following table represents the results :—

Ratios of Maximum Hogging Moment. + Maximum Sagging Moment.			
Reference Number of Ship.	Buoyancy calculated as explained in the Paper.	÷	Buoyancy supposed proportional to Volume of Wave displaced.
1	100	÷	170
2	100	÷	165
3	100	÷	155

The differences here are of considerable amount, and, provided the method explained in the paper is not largely in error, show us how enormously on the safe side we have been in the habit of computing these moments.

It is implied by the title of the paper, but it may be necessary to state explicitly, that I am not attempting to take all the circumstances that affect the strain on the ship into account. For reasons stated at length farther on, this course appears to be impracticable. I am simply following to a conclusion a certain reasonable assumption as to the buoyancy afforded by the waves supporting the ship. The effect of the up and down motion of the vessel in altering her virtual weight, for instance, has been altogether omitted. This, however, from rough estimates I have made, can never be very great, as regards longitudinal strains, in the case of a ship steaming at right angles to the wave ridges. If the vessel were broached to in the trough of the sea, the alteration of virtual weight might be considerable, and would have an important influence both on the transverse and longitudinal strains. The longitudinal strains, under these circumstances, would be simply the still water strains increased or diminished approximately in the same ratio, that the virtual weight exceeded or fell short of the actual weight of the ship, and these strains are not nearly so great as the strains brought about by the vessel steaming head to sea.

When the vessel is steaming head to sea, there is much less variation in her virtual weight than when steaming on a course parallel to the wave ridges, and this is especially so in the case of full ships, *i.e.*, of ships experiencing the greatest changes of hogging and sagging strains when in a sea way. On account of the smallness of this feature, when the longitudinal bending moments are at their greatest, I have therefore altogether neglected taking it into account.

Figs. 1, 2, and 3 (Plate IX.) show the influence of variations of draught of water in affecting the difference between the results obtained by the two methods.

I have thought it sufficient in this case not to labour with actual ships but to take box-shaped vessels, as the general tendency of the figures must of necessity be the same, and

the figures themselves cannot be far from the truth. The following are the results when floating on the wave :—

	Maximum Hogging Moment + Maximum Sagging Moment.		Ratios of Maximum Hogging Moment + Maximum Sagging Moment.	
Mean draught of Box.	Buoyancy calculated as explained in the Paper.	Buoyancy supposed proportional to Volume of Wave displaced.	Buoyancy calculated as explained in the Paper.	Buoyancy supposed proportional to Volume of Wave displaced.
10′·6″	10	125	100	125
20·6	83	125	100	150
25·9	75	125	100	165

These figures show that as the draught increases, the exaggeration of the bending moments caused by not taking the wave structure into account rapidly increases also, and a little reflection will suffice to show that if we duly allow for the wave structure we shall ultimately, as the draught of water increases, get to a condition of things in which there is no bending moment at all brought upon the ship by the passage of the wave, the bottom of the ship being so far down in the wave as to experience no variation of pressure throughout its whole length as the wave passes along it. If we make no allowance for wave structure, but suppose the buoyancy at each section of the ship to be proportional to the displacement at that section, we shall be supposing the same bending moment to exist as when the draught was shallow, whereas there would be a total absence of bending moment.

In this way a vessel of great depth from keel to upper deck gains doubly over a shallow vessel. On account of the greater depth of the section she is more able to bear the varying moments that do occur than the shallow ship is, and, on account of her deeper immersion, those variations of bending moments are of less magnitude than in the shallow ship.

As the draught diminishes to nothing, the two methods of calculation lead to identical results, and it is therefore immaterial which is adopted.

The principle of calculation adopted in the paper is certainly open to some question, because it supposes the wave structure uninfluenced by the intrusion of the ship, and for a ship as broad in proportion to her length as the *Inflexible* is, there may possibly be a considerable error in the assumption, but for ships such as recent merchant ships, which are narrow in proportion to their length, and therefore narrow in proportion to the length of wave on which they would be supposed floating when calculating the hogging and sagging moments, this source of error probably would not be so great. There are other objections of a minor nature that need not be specified. Enough, however, has been brought forward to show that very different results may be obtained by looking at the question from

different standpoints, and also that part of the difficulty we have all experienced in understanding how a ship has borne so well and without signs of distress the strain per square inch of material that was said to be imposed on it, may be due to the strain not having been there at all. This strain, at any part of a ship's section, depends upon the bending moment and other forces acting at that section, and also upon the nature of the elasticity possessed by the materials forming the section as a whole, viewed in the light of what is usually called the "equivalent girder."

There are difficulties in this part of the subject foreign to this paper, but there is little doubt that at the best the ordinary method of assuming perfect elasticity can only give a coarse approximation to the distribution of the strain across the material forming the ship's section, even if we knew the exact value of the bending moment operating at the section. The method of estimating the bending moment on any section, as explained in the paper, omits many considerations, and the method adopted up to the present is still more imperfect, and under this double uncertainty—uncertainty as to the bending moment, and uncertainty as to the distribution of that bending moment across the ship's section —it is impracticable to predict, by mere calculation, the actual strain on a given part of the ship's structure, even under known conditions of sea and load. What is required is careful watching of ships, especially those that are weak longitudinally. In the Admiralty Service we have no chance of gaining this knowledge, on account of war ships having so much material in them for purposes other than those of structural strength. In the Merchant Service, this knowledge, even when obtained, is not available to the profession at large, on account of the natural desire of builders and others not to publish details of failure.

There is this further difficulty—a difficulty which is perhaps peculiar to questions of strength—that unless the structure breaks down, we only know that it is strong enough and not how much strain is on the material. If the structure breaks down we only know that it was too weak, and not by how much.

In other questions, such as propulsion, for instance, if there is a failure we have a numerical value of the failure, and this accurate knowledge of the shortcoming is available for guidance in future work. At present we have no such knowledge with respect to the strength of ships. To supply this knowledge we must have instrumental observation of the strain and variation of strain that goes on in the different parts of the ship's structure. Such instrumental means, perhaps, may never be obtainable. A small machine, however, was placed in the market some time ago, and known as Stromeyer's Strain Indicator, that bade fair to accomplish this kind of work. The principle of the machine depended on the creation of Newton's rings, and for some work the delicacy of the machine is marvellous. Some of the members present may have had experience of the machine on shipboard in a sea way, and be able to say how far it is applicable to the work indicated above. It would not be difficult, I think, to utilise the varying resistance of some substances to the passage

of a weak current of electricity, as the compression on the substance varies, to perform work of this kind. The disturbance of a delicate galvanometer by the slight motion of powerful electro-magnets to and from each other when attached to any part of the structure under strain might also be utilised. The readings in such instruments would be easy to deal with, and might be made self-recording.

In conclusion, I would say that although we have had numerous abstract calculations of hogging and sagging strains put before us, there is a singular dearth of information of *actual* strains on ships, whether these are deducible from cases of failure or otherwise; and if my paper should elicit knowledge of this sort, either now or at some future time, after careful experimenting, its object will have been served. Next to questions of propulsion, I think this is one of the most important matters to take up. In ordinary ships the power to propel them at a given speed can be predicted within very small limits of error, their stability can be accurately calculated—their strength and the strains on them cannot be calculated with any pretence to precision.

DISCUSSION ON THE TWO PRECEDING PAPERS.

Mr. W. John: My Lord, I rise to say at once that in my humble opinion this paper is a very valuable one. Mr. Smith has taken a step which is a natural one, and the natural outcome of the development of our knowledge of wave motion. It amounts to this, and it is a step analogous to what was made by Mr. Froude and others in reference to the rolling of ships. In the first place we get a knowledge of wave formation in reference to surface motion. Mr. Froude's first calculations of the rolling of ships were based practically upon the wave slope, but he almost immediately, or in his very first paper, made a reservation to this effect, that there was an effective wave slope farther down which would be not quite so steep, but which, when found, would be the real governing quality. That is a simple illustration of the step which Mr. Smith has made from the calculations that have preceded him. I have made, and other friends of mine, and Mr. Smith have made, many calculations upon wave motion and the strains of ships, knowing there was a certain element of error which we and Professor Rankine and others allowed for in our general ideas of our margin of safety, and Mr. Smith is the first who has put it practically and tried to test it quantitatively. That is really the sum and substance, and the step Mr. Smith has taken in advance. I, and many of us, have seen the thing, but we have not tackled it; we have not calculated it. Mr. Smith has, and although I cannot check his figures, I have no doubt they are correct, and in principal I have not the slightest doubt as to his accuracy. I have also very little doubt as to the accuracy of his figures quantitatively, and I am always very pleased to see a step in advance beyond what any of the rest of us have had either the time or the opportunity of taking. The next point I come to is Mr. Wigham Richardson's paper, and I wish I could speak as generously of his paper as I can of Mr. Smith's, but I am really not prepared to say that Mr. Wigham Richardson's paper adds one iota to the knowledge of this Institution, or to the statistics that will go into the records of this Institution, or will add in any shape to the value of our knowledge of the strength of ships. Mr. Richardson read a paper here a few years ago which I had the misfortune to have to criticise unfavourably. The present paper is an advance upon that paper to this extent, that it does not contain so many fallacies

as that paper, but I am sorry that I cannot say that substantially it contains any more really valuable data. Before going further, I would like to say a word about what passed this morning. Sir Edward Reed quoted a figure of mine about the strength of ships, and said (speaking from memory) that Mr. John had shown that the factor of safety of mercantile ships was only two. Well, I ought to have got up immediately after Sir Edward Reed and corrected him, but you were pressed for time, and you immediately called on Mr. Milton, and that is the reason I did not get up. But really the facts of the case were these; this is simply an explanation.

The PRESIDENT: Do not revive a controversy which is over, and especially if you can avoid it, as Sir Edward Reed is not present. There is an obvious inconvenience in reviving controversies in any case; therefore, for the sake of the regularity of our proceedings, please make it, at least, as short as possible.

Mr. W. JOHN: I will only say this, that it is a little correction that Sir Edward Reed, if he were present, would be the first to get up and approve of. It was simply this: In my paper that he referred to I showed that as to ships in the Mercantile Marine, as they got larger, the factor of safety got gradually reduced from five or four down to about three, until it became really a matter of serious concern, and Sir Edward Reed simply took the figure as two instead of three. The only other point I will mention is this, that that factor of safety is really not a working factor of safety, because it assumes certain conditions of wave formation, viz., that the wave shall be the exact length of the ship, a circumstance which can only occur, perhaps once or twice in the course of a few years, and it is really not an every day working condition of the ship; it is an exceptional factor of safety, and it is one that sometimes occurs, and sometimes produces strains in the ship, which you see when they come into port, and with which they go out again without being strengthened, then these ships do not fall into the same circumstances, and they do not meet with them again, perhaps for several years. I wanted just to put that explanation on record, and I am sure if Sir Edward Reed were here he would be the first to say it was quite correct. Now I come back once more to the question of Mr. Wigham Richardson's paper, and to the question of Lloyd's Rules. I think, if Mr. Richardson would really take the matter up from a proper scientific standpoint, and calculate on the principles of the equivalent girder, or upon any recognised mechanical principles for ships of different sizes according to Lloyd's Rules, he would find they were not in the condition in which, I am afraid, he wishes to convey they are. Now when I spoke last in reply to Mr. Richardson I was a member of the executive staff of Lloyd's, and I may then perhaps have been assumed to be prejudiced in their favour. I speak now from the point of view of a fellow shipbuilder of Mr. Richardson's, and I am free to say that, if I can find any holes in Lloyd's Rules, I am quite prepared to put them forward in this Institution on scientific grounds, and to defend them against my friend, Mr. Martell, or any of my other late colleagues, but I will only do it upon fair scientific grounds; and I say this, that although as to Lloyd's numerals, or the Veritas, or the Liverpool Registry, you cannot defend them upon a scientific basis, the great mistake often made, and the one made by Mr. Richardson, is this, in assuming that those numerals are really in proportion to the scantlings of the ship. They only form a basis, and if you come to go through the Rules, you will find they have been built up. The scantlings are not in proportion to the numerals, but the scantlings are based upon practical experience in such a way that it is an extremely difficult thing for the most scientific man in the country to absolutely show the connection between the scantlings and the numerals, and to prove scientifically where the one is right, or where it is wrong. And I conceive, with regard to Lloyd's Rules as they stand at present, though I do not approve of them in many respects, if you could put them into a formula it would be a very com-

plicated formula, but it would be a far more nearly accurate formula for the strength of merchant ships than any other scientific formula I have ever seen. However, I hope that our friends at Lloyd's will recognise the fact that it is a difficult problem, and will act in conjunction with shipbuilders, and try not to pile on strength too much, and make themselves too safe, as we heard this morning is the tendency of gentlemen in official positions. Subject to that, I think they may be quite prepared to face this Institution, or any other public institution, and I do not think, with all deference to my friend, Mr. Richardson, that a paper of the kind that his is, is the one to shake public confidence in them.

Mr. B. Martell: My Lord, I should like first to refer to one paragraph in Mr. Richardson's paper to begin with. Mr. Richardson refers to a small vessel which he says was intended to carry little or no cargo, and though she had done her work remarkably well, he says that unfortunately the Register Society—I do not know whether his remarks apply to any other register society than Lloyd's—but I know an application was made to Lloyd's Register Society for a similar vessel, and therefore I will assume it was Lloyd's Register Society.

Mr. W. Richardson: May I just correct Mr. Martell, and say that he is in error.

Mr. B. Martell: I will only say then that as to one or two similar vessels which were about to be built, inquiries were made for the purpose of having them surveyed whilst building by Lloyd's surveyor, and Lloyd's Committee, I think, very properly refused to have anything to do with them. And it was for this reason: the scantlings were exceedingly slight. They were intended for coasting purposes—I believe on the coast of America—and though, probably, when built, they would perhaps be very suitable for a trade of that kind, the inspection of the surveyors to Lloyd's Register Society was required merely in order to get a certificate, as I imagine, for the purpose of insurance in going out there. There is a very great difference, I need not say, between crossing the Atlantic to go to a place like that, and a vessel performing her work when she gets there. The Committee thought it was essentially a question between the owner of the vessel and the underwriters, and consequently refused to have anything to do with the vessels under such circumstances. That is merely by the way. I may be wrong, as Mr. Richardson says, as to the actual vessels, but still that is my explanation, as far as my memory goes, of a similar case. Now, with reference to this paper generally, I think it is very much to be regretted that Mr. Richardson has not given us a little information as to the scheme he would propose as the basis on which he would frame rules for the building of iron ships. He has not indicated that to us. In the paper referred to by him that he read here in 1875, he did indicate to a certain extent what he would propose to do, but those propositions were found to consist so much of what Mr. Richardson thought to be proper, and what he assumed to be right, and were altogether so wanting in a scientific basis, that that paper was very severely criticised, as Mr. John has said, on that occasion, and I certainly cannot imagine that after that criticism Mr. Richardson has not altered his opinion as to the principle of framing rules for building iron ships. Therefore I regret, seeing that ten years have now elapsed, that we have not at the present time the advantage of the improved knowledge that I assume Mr. Richardson must have acquired after the criticism on that paper. However, we assume from this paper that Mr. Richardson considers the basis of scantlings for iron ships should be framed on two elements, that is, the element of displacement and the element of length; and he appears to take it for granted that these two elements are the only elements that enter into the condition of strains of ships. Well, if he would prove that to us—(which he has not attempted to do, because he has only told us that "no one who has considered the thing at all will but accept that as a fact," which I

most distinctly do not, and I believe there are many here who have gone into the subject who will agree with me that they do not believe anything of the kind)—if he had proved that in any way, or attempted to prove it, or if he could explain to us that it was at all approximately correct, then I say that the basis of rules for iron ships could be taken so that it would be a scientific basis upon which we should all agree, and be perfectly willing to accept. But that has not been done, and I can only say that so far as my experience goes from having seen ships after they have performed long voyages, heavily laden, those ships have, from encountering exceptional storms, and being very fully laden, shown symptoms of transverse straining, which length and displacement alone would not account for. I do not at all accept the statement that the displacement and the length are the only two elements that enter into the conditions of the strains of ships, and I am quite sure I could convince Mr. Richardson of that if I had the opportunity. And here I may say that not many shipbuilders have the same opportunity as surveyors of seeing a large number of these ships coming home from long voyages. Well, not only does experience not show that this assumption is correct, but scientific calculations show quite the contrary—that these are not the only two elements, as shown by calculations made by men of great mathematical attainments, who have studied this subject on a perfectly scientific basis. Now, only last year, my Lord, we had a paper read by Mr. Read and Mr. Jenkins (and I am sure the flattering manner in which that paper was received, and the high eulogiums which you passed upon those two gentlemen, will bring it to the memory of all who were here on that occasion) on the transverse strains of ships, and they conclusively showed in that paper that not only have you to consider longitudinal strains, but that, likewise, you have to consider to an equal extent transverse strains, and that even in an iron ship placed in a dock and supported by blocks all fore and aft, with no cargo in her, the transverse strain to which such a vessel is subject is not either small or inappreciable. I am very happy to inform the Institution, because I am sure you will all be pleased to hear it, that those investigations have been still further continued by those gentlemen, and I only regret that time has not allowed them to complete them, so that another paper on this subject could have been read on this occasion; but they have been following up this subject, and so far they have arrived at a result in investigating the strains to which ships are subjected in crossing waves in large seas under certain conditions, that the transverse strains nearly, or quite equal in some instances, the longitudinal strains, and, mind, these calculations have nothing whatever to do with the length of the vessel. Therefore, to assume that the displacement and the length are the only two elements entering into the question you have to consider in the framing of scantlings of ships, shows that insufficient consideration has been given to the subject; neither does it commend itself to us to admit of our accepting this assumption, and altering the Rules from the basis upon which our scantlings at present are formed. Mr. Richardson at the same time refers to the Veritas Society Rules, and there he shows that those Rules are based on a fallacy, because they deduct three-tenths from the product of the length multiplied by the breadth multiplied by the depth in framing a rule for their scantlings. Well, I think we can all agree with that—I do, at least; but it is a very remarkable thing that Mr. Richardson seems to fall into a somewhat similar error in criticising the basis on which Lloyd's Rules are framed. It seems to me there has been a want of proper care in looking into the subject, because, to assume that the numerals on which the scantlings of Lloyd's Rules are based are synonymous with the scantlings given, as Mr. Richardson does, is, I need not say, my Lord, altogether wrong. Everyone who understands the subject at all knows this is not the case, because when you come to the large ships—take, for instance, such a ship as the *City of Rome*—you will find in the numerals a difference of 10,000, whereas in the smaller vessels there is a much smaller difference between the numbers for scantlings, for vessels of different

sizes. If you have in the small ones $\frac{7}{16}$ths of an inch for the thickness of plating, you would, by taking the difference in the numerals, want *ten* times as much when you come to the large one, or, say, $\frac{70}{16}$ths; that is, you would require all the outside plating, according to Mr. Richardson's reading of the Rules, to be $\frac{70}{16}$ths all throughout the ship. It must be evident to all that that cannot be the principle on which these Rules are based. I should be very happy if Mr. Richardson will honour me with a visit to explain to him how it is done, so that he shall not fall into an error of that kind on any future occasion in bringing forward papers of this kind. In the next place, Mr. Richardson says that if you take two exactly similar ships, the one twice the dimensions of the other, the displacement—if the draught of water be also doubled—would be increased as the cube of two, that is, eight times, whereas the numeral in Lloyd's table of scantlings would only be doubled; that is to say, that the scantlings would increase as the squares while your ship would increase as the cubes—that is what I understand; is not that so?

Mr. W. Richardson: I think not.

Mr. B. Martell: Then what is it?

Mr. W. Richardson: The first power, I think, you will find in the paragraph. You are putting it in your own way.

Mr. B. Martell: I do not want to take any unfair advantage in any sort of way, but you say if the ship is increased twice the dimensions and also the draft of water, that the displacement would be increased eight times, whereas the numerals would only be doubled, and you say the effect of this, as I understand you, is to produce a strain eight times as much on that double ship. Well, that really appears to me to mean, in plain language, that if you have five tons per square inch of stress (and you say you base your calculation on that) on the one ship, you will have forty tons per square inch of stress on the other. It seems to me that that is a very unsound conclusion. I do not want to occupy the time of the meeting too much, but I will only say a word or two with reference to the curve placed before us. I must say that I regret very much to see that curve produced on this occasion. On the first occasion, in 1875, it was produced incidentally—Mr. Richardson held it up in his hand, if I remember rightly—and the feeling of the meeting was that it was not fair to produce under those circumstances a curve such as this, and that until the members of the Institution had had an opportunity of looking into it and seeing how it was constructed, it ought not to go into the Transactions of the Institution. Now, it only shows the wisdom of not admitting things outside the Rules of the Institution, because I think if we had admitted that without any explanation whatever, we should not have placed this Institution in a proper position. We have a curve now put forward, and although we have not a distinct explanation of how that curve is produced (and I am sorry that it has not been, because I think we ought to have had it), we can, on reference to this paper, judge for ourselves how it has been produced. Mr. Richardson, in the first place, takes four ships from 200 feet up to 400 feet in length, and by his rule of taking the square of the length into the displacement he arrives at certain strains.

Mr. W. Richardson: No.

Mr. B. Martell: I believe that is it.

Mr. W. Richardson: No.

Mr. B. Martell: I have assumed that to be so; it seems to me to be the only legitimate construction to put upon it. Perhaps Mr. Richardson will kindly explain how it has been constructed?

The PRESIDENT: Order! order! Mr. Wigham Richardson will have a full opportunity of replying.

Mr. B. MARTELL: Very well, my Lord. I assume that that is how it is done. He arrives at certain results as to the strains on these ships of different lengths. I leave out the very abnormal proportions assumed for these ships. You have a ship 400 ft. long with 34 ft. beam and 25 ft. deep! Why, that is something that nobody would ever think of building for sea-going purposes; but still, we will take it as it is, and assume these ships are put on a bank, as Mr. Richardson says, and the displacement is multiplied into a fraction of the length, and you get certain strains. Now, if I understand anything about calculating strains, I must assume that Mr. Richardson means the bending moment. That, I suppose, is his meaning, although he has not said it; he gets at certain bending moments, and, as I understand, calls these *strains* at the gunwale, but that is a term which is altogether Mr. Richardson's own—it is not understood by any scientific writers on the same subject, because he leaves out altogether, in calculating this, the most important element in arriving at strains of this kind, the moment of inertia. That is ignored altogether in the matter. He arrives here at the strain by taking the displacement multiplied by a function of the length, and in some way that is assumed to represent the number of square inches of iron in a ship. It does not do anything of the kind; it is nothing of the sort. A parabola, in fact, is constructed by taking the length of the ship as the base line, and some multiple of the square of the length as the vertical line, and he gets that curve. Well, what does that curve show? It shows this: that if you had a ship 100 ft. instead of 200 ft. long, by arranging a system of that kind for the plating of ships, you would have a vessel 100 ft. long with less than one-fourth of an inch thickness of plating, and when you come to a ship like the *City of Rome*, you would have the outside plating three inches thick all throughout the ship! Well, that I need not say is altogether impracticable as far as ship construction goes. The great fault, it appears to me, in this paper, is that the *distribution* of material is entirely ignored. You might have a small amount of material so distributed as to make a ship longitudinally strong, but exceedingly weak transversely. We remember the previous paper which Mr. Richardson read. I believe it was there really advocated to do away with stringers in the ends, and it was shown by calculations made in this way, the less strength you have in the ends of the ship the stronger the ship is. I remember that paper perfectly well, and this paper appears to be founded on the same principle. If you obtain what is called the strain on the ship at the gunwale, that is, make a parabola like that, calling the bending moment proportionate to the number of square inches of sectional iron in it, and calculate a curve as shown, you will get a very extraordinary result, because, as I said, you will get a curve showing you want plating three inches thick in very large ships; while in ships we are building so many of to-day, you could do with plating less than $\frac{4}{16}$ of an inch thick. Well, I think that shows it is a very improper and inaccurate way of constructing such a curve for that purpose. I am sorry I have exceeded my time, but I thought it rather important that we should explain pretty fully, as far as time would admit, our views with regard to Mr. Richardson's paper; because, when a critic comes forward in order to give us all the information he can for the purpose of improving our Rules, we do not like to treat him with disrepect; we like to show whether there is any foundation at all for the proposals and arguments he advances.

Mr. H. H. WEST: My Lord and gentlemen, as this paper of Mr. Richardson's mentions the Underwriters' Registry, I should like to be allowed to say a few words about it. I would venture to remark before I say anything further, that I think Mr. Martell has read into the paper what is not in it, and has addressed his criticisms to what Mr. Richardson did not say—that displacement and length were the *only* things to be considered. As far as I can see, Mr. Richardson has not said any such

thing; and I do not think he or any member of this Institution would propose to build ships exclusively on this basis. In reference to the displacement basis, I think Mr. Richardson has hardly done the Underwriters' Registry justice in saying that length is practically ignored by them. Now, the "plating" basis of the Underwriters' Registry is essentially a displacement basis, that is to say, it is a tonnage basis, and tonnage is a fair measure of displacement in most cases; the cases where it is not a fair measure are so easily dealt with, that it is unnecessary to make any further comment upon them here. But there is a further element that is taken into consideration in the regulation of the "plating," that is, the ratio of depth to length; and I think our displacement basis with this modification does deal with the two points that Mr. Richardson makes such a strong case about. In reference to transverse strains and so on, to which Mr. Martell refers, we are all alive to the importance of them, and they are not neglected in the scantling arrangements published by the society with which I am connected. I would only say further, that I think Mr. John, when he put forward his paper in the Transactions for the year 1874, which is referred to here, limited the application of his method to what is practically a statical problem; and Mr. White in his book, in dealing with that particular method, also limits it. I would commend to the careful and thoughtful perusal of members of the Institution a paper read by Mr. Mansel before the Institution of Scottish Shipbuilders, on the subject of longitudinal sea strains as indicated by Lloyd's experience. He proceeds on the basis of accepting Lloyd's Rules as indicating what has practically been found necessary, and he tries to deduce from that a wider and more general law. The paper is well worthy of careful consideration, and I am only sorry it does not appear in our own Transactions.

Mr. W. H. White: My Lord, I am rather sorry that the two papers have been read together, because in the large attention that has been bestowed on Mr. Wigham Richardson's paper, Mr. Smith's paper has been, except for Mr. John's opening remarks, almost passed by. I am not going to criticise Mr. Richardson's paper at all, but I do think that this paper of Mr. Smith's is worthy of the very careful study of all who take an interest in the subject of the strains of ships. Mr. John has summarised, I think, the principles of the paper very fairly. Of course, it must always be remembered that between the idea of an omission and the quantitative expression, or even the approximatively quantitative expression of what that omission may mean in a calculation, there is a very wide step. Now, Mr. Smith in this paper has shown us that the omission of variations in fluid pressure due to wave structure and wave motion may have the effect of practically reducing the bending moment in the ratio of 100 to 145, that is to say, in that same ratio increasing the factors of safety which would appear in the ordinary method of calculation. Of course these factors of safety, as Mr. John very properly says, are not working factors of safety. In saying that Mr. John endorses what Mr. Smith says at the end of his paper most strongly, that is, that we want to have recourse to the observation of the actual behaviour of ships, their actual straining or indications of strain, in order to know up to what strains calculated on the girder hypothesis you can safely go. I think that that is the conclusion which will be reached by everyone who has looked into the matter. I do not think that for purposes of comparison between ships which may fairly be compared, that is, ships of approximately the same size and of a somewhat similar class, it is necessary to make this correction upon the usual method. I do not understand Mr. Smith to mean that he would do it in ordinary cases. But he has here put before us what it may amount to, and what may be worth taking heed to, if you are going to treat these factors of safety as more than they should properly be treated. It is important to notice that in a good many of the estimates for these factors of safety—I am speaking from memory, but I think the remark applies also to the calculations Mr. John made

—very flat waves have been taken, flatter waves than are often met with, and in that way Mr. John in making his calculation—I think $\frac{1}{35}$ was the factor which he used—really did take an effective wave slope, in effect a wave somewhat flatter than the actual wave slope which would be met with commonly in practice. I do not think there can be any doubt that if ships were fully strained up to the limits which were indicated in some of the examples given by Mr. John, but that there would be much more frequently failures than really happen in practice. I think that was the conclusion Mr. John himself stated at the end of that very paper. I know if he did not then he did in a later paper, that you might really have strains indicated on the girder hypothesis, with the ship across the crest of a wave, of eight or nine tons per square inch of sectional area, and yet in actual practice you might find those ships going about the world for years without indicating any straining of a serious character.

Mr. W. John: The higher ones were really showing signs of strain.

Mr. W. H. White: The strain where signs of distress appeared was very high—much higher than would be adopted in any engineering structure. But I cannot help thinking that what Mr. Martell said in reference to Mr. Wigham Richardson's paper has a very wide application; that is to say, that, after all, in by far the greater number of cases with which one has to deal in providing for local strains, the principal strains are really met. That is a very important matter, which largely affects any scheme of scantlings based on any general formula. I do not fancy it would be possible to use any such instrument as this strain indicator which Mr. Smith suggested with very satisfactory results, and simply for the reason—although I am only speaking an opinion, and it may be wrong —that you have in so many parts of the ship materials of a different character acting together, more or less, such as the wood covering to a deck which may be plated with steel or iron—and I do not quite see how you can apply a strain indicator under those circumstances. If you have a bare deck and all the materials of one description, then it is quite conceivable that it might be done.

Mr. W. John: If you can hit the shear streak you can get at everything.

Mr. W. H. White: It may be possible to apply it, as Mr. John says, to some critical part of the structure such as the shear streak; it would be interesting, at all events, to have that observation, because in many ships of war, whatever it might be in merchant ships of a simple structure, I am afraid it would not be possible by the use of such an indicator to get at any result to be fairly representative of the condition of strain in the critical parts of the structure. I cannot help thinking that Mr. Smith in this paper has really made a most valuable and eminently interesting addition to anything before furnished on the subject.

Dr. Woolley: My Lord, I do not intend to occupy your attention long at this late period, but I cannot allow such a paper as Mr. Smith has contributed to pass without expressing my great thanks to him and my congratulations to the Institution upon the importance of the paper having been produced. It is very true, as Mr. Smith has pointed out, that the structure of the wave upon which it is founded has been known from the very beginning of these investigations. I remember in Mr. Froude's first paper he particularly points out this character of the structure, which he calls, in fact, the tension of the wave, and we know that at the top of the wave, for instance, the gravity has to be reduced by the whole amount of the centrifugal force, while at the bottom of the wave it has to be increased by the same; but this is the first occasion that I know of on which any practical attempt has been made to apply those principles to the calculations of the strains which are brought to bear upon ships, and, therefore, I think it is, as has already been observed by Mr. John and Mr.

White, a very great step in advance. The trochoidal form of the wave in which these principles enter is the basis of the process adopted by Mr. Smith. I can only say, as one of the oldest members of this Institution, that I have heard this paper read with the greatest pleasure.

Mr. P. Jenkins: My Lord, I think that in attempting to correct Mr. Martell, Mr. West has, in some form or another, fallen into an error. If you turn to page 132, you will find that Mr. Richardson speaks of hogging, sagging, wracking, and torsional strains; and he says that these are derived primarily from the displacement and from the length, and further on in the same paragraph he says, "It is sufficient to show that in fixing the scantlings of the midship section, the principal stress will always depend upon the displacement and the length." Therefore it is evident that if the principal stress is dependent upon the displacement and the length, then you have no other element entering into the determination of the scantlings, and I think Mr. Martell was quite right in making the statement he did. I want now to refer to Fig. 7, Plate VIII., which Mr. Richardson has upon the wall. I think that, in producing that diagram, Mr. Richardson has used a formula which he ascribes to Fairbairn, but which, I believe, was intended by Fairbairn for an altogether different structure than a ship. I think that Fairbairn's formula was more applicable to a girder in which the web was not an important element, and in which the web could have been altogether omitted. In such a case, part of the material might be assumed to be collected at the centre of the top flange, and part of the material at the bottom. In that way you get the stress proportionate to the area of the material in the section of the girder; but when you come to a ship it is altogether a different thing. In a ship the web of the girder is a most important element, and if you attempt to leave out that element, as Mr. Richardson has done——

Mr. W. Richardson: No.

Mr. P. Jenkins: I beg pardon; I find, by making a rough calculation, that Mr. Richardson takes the area of the material in the midship section of the vessel, and makes this area proportional to the bending moment when the vessel is upon the rock. In such a case as Mr. Martell has pointed out, it is plain that, with 2,098 square inches in the section of the vessel, one thing only can happen, and that is, that you must make the thickness of the bottom plating so much that the vessel would actually sink under her own weight—the vessel would not float. If it were not for that fact I would recommend to the gentlemen I see here from the Admiralty that, if they hear of such a ship being built, they should take care to add that ship to the list of vessels available for the purposes of warfare. I would say one word further with regard to a vessel of that kind being poised on the top of a rock. It is very evident that the first thing that would happen in practice in such a case would be that the bottom would go up: the shear strake would remain intact, but the bottom of the ship would be entirely turned up. Now I will turn to Mr. Smith's paper, and I have nothing but commendation with regard to it. I myself have been making some calculations lately regarding the transverse strength of iron ships, and I found (in conjunction with my colleague, Mr. Read) that it was very necessary to take into consideration the variation in pressure, which is different from that in still water. We had only to deal with the wave structure at the hollow of the wave and at the crest, and we found that it was sufficient for all practical purposes to suppose the water at the wave hollow made denser throughout than in still water (I think the difference in the wave which we took was something like 13 per cent.), and that the wave crest in the same way would be lighter. Mr. Smith has not given us actual figures upon which we can calculate how much the stress on the gunwale of the ship would be reduced by taking into account the wave structure, but I think, upon looking at the diagram, it would be reduced by about 25 per cent.; and I think we must all admit that a reduction of 25 per cent. from the stress of the gunwale

of a merchant vessel is a most important consideration, and, if anything, it tends to show that longitudinal strains are not necessarily the principle strains that vessels are subject to. My own investigations have for some time led me to that conclusion, and the investigations which Mr. Smith has brought forward on this occasion tend to confirm me in that opinion.

Mr. A. J. Maginnis: My Lord, I should like on this subject to lay before the meeting to-night a matter which has often struck me, that is, are those who are constantly engaged in getting merchant steamers ready for sea, to go into these calculations? Speaking for myself, I have no time to go into them, and I would ask to-night if it is not possible that, in some concise way, the results of all the beautiful formulæ and scientific calculations which are yearly brought before this Institution, can be put into a plain practical form, so that at, say, a quarter of an hour's notice, men who are suddenly called upon to apply them, can just turn them up and see at once whether they are applicable or not? I must say that last year, judging from the brief notices I had seen of the papers brought forward by the last speaker and his colleague, it was with extreme pleasure that I looked forward to the volume of Transactions of this Institution, and for this reason, that I have often known cases where very large merchant steamers have been suddenly required to be placed in a dry dock, and the first question the shipowner will then ask his professional or practical man is, "Is that ship safe to go into a dry dock with such an amount of cargo on board?" Now as the matter stands at present, I think there is not—I say it with very great reservation, indeed—one in the scientific world who can answer with any certainty, from all the formulæ calculations or diagrams ever yet brought before this Institution, and say, "Yes, that ship is perfectly safe with 4,000 or 5,000 tons of cargo on board." There is no doubt that with these elaborate diagrams we are getting in the right direction, but I would say to these gentlemen, drop for a time your formulæ, &c., and put them into a practical form, so that people who are suddenly called upon in these cases can refer to them at once, and see how to act. I will mention what happened to me a short time ago, when I was carefully studying the paper of Mr. Jenkins, read last year. In the midst of the calculations I was called away by a telegram to attend to a very urgent duty indeed, in connection with a steamship about to sail with a considerable number of passengers on board, and up to that time I had spent more than two hours over the paper, and could not up to then apply it to the case I wished; and I would ask whether it is not possible to put these things into such a form that a short interval will enable us to make use of them? Then, again, I cannot say how much I think of the statement that Mr. Smith has put forward to-night, that is, how little we know of all the strains that are brought upon our steamships. No doubt everyone who has gone through the yearly Transactions of this Institution will have seen, as we have seen to-night, magnificent diagrams, with strains and everything beautifully worked out on paper, but I would to-night—speaking in utter ignorance, for rarely do I come to these meetings—ask Mr. Smith and Mr. Jenkins, and others, at what portion of a ship's voyage are these strains calculated for? It must not be for one minute thought that strains on a ship are always the same. I know there are members here to-night who think that I am speaking very strangely, but what I would say is this: are the strains on steamers leaving port heavily laden throughout with coal and cargo the same as when they have been out ten days and burned 1,000 tons out of the most vital part of the ship? This matter was brought home to me very forcibly a short time ago. I found in my experience that the ships in the short portions of the voyage that I made with them were stiff and rigid, and everything that ships should be; but, to my astonishment, the men who were in these ships afterwards reported to me things occurring which, unless I had seen them in other ships at sea myself, I could not have credited, and I am certain no members of this Institution would. I am sorry to say I cannot give

anything in the way of particulars, but the simple way of explaining it is this, that when you have burnt 1,000 tons out of the middle of the ship, you have only the sectional thickness of the plating, &c., to support the parts which have to take the greatest strain in large steamers. This matter was brought before me a short time ago in connection with a sailing ship in this way: the lower deck stanchions had carried away at sea, and when they got into port and discharged cargo, the captain stated that the lower deck beams had sunk 3 in., and yet were uninjured. I need scarcely say in a ship of—speaking from memory—about 30 ft. beam or so, and I think the members of the Institution will agree with me, that it would be almost impossible that they could have sunk 3 in. without showing some weak point or buckling in some way; however, I understand the captain spoke exactly what was the fact. Screw-jacks and other means were used to force these beams back into position. My conclusions were that they forced the deck of the ship up, and the bottom of the ship down. I know there are many members of the Institution who will not believe that, and I may be in error. I never have had the means of making any calculations, but the captain on examining the keelsons of the ship found them intact, and also found that the stanchions had given way nearly 150 feet. I think that these calculations are certainly tending in the right direction, but I would ask the members of the Instituton to bring them before those who have no time to go into them in a concise form, that they may make use of them.

Mr. T. C. Read: My Lord, Mr. Richardson has objected to the explanation which Mr. Jenkins gave as to the method by which the thick line in Fig. 7 (Plate VIII.) was obtained. He also said in the course of reading his paper that the way he had obtained it was by Fairbairn's formula, and that the difference between the results obtained in that way and in the way they were usually obtained would not be very great. Well, I have taken the cases of four of his vessels there, and have estimated the variations of the stresses as the ship is lengthened, and I have also taken the black line which Mr. Richardson has given there, and if the ordinate of the black line is divided by the ordinate of the line, which is stated to give the area of the plating in a ship built according to Lloyd's Rules, the quotient multiplied by five would give the stress on a ship built according to Lloyd's Rules. I have, however, not gone into absolute stresses; I have taken the stress of a ship 250 ft. long as 100. According to Mr. Richardson's figures, when the ship was 300 ft. long it would be 130, when the ship was 350 ft. long it would be 152, and when the ship was 400 ft. long it would be 167. Now, by taking the moment of inertia of these sections, I find that the 300 ft. ship would have a stress of 118 instead of 130, as obtained from Mr. Richardson; the 350 ft. ship would have a stress of 122 instead of 152; and the 400 ft. ship a stress of 125 instead of 167. These are very considerable differences, and show in a striking manner the effect of an efficient distribution of the material. I think, if I may be allowed to answer Mr. Maginnis, I may say that in all the cases my colleague and I worked out last year for the paper on transverse strains and laid before this Institution, we gave the results in the diagram in such a way that they could be measured off; and next year, if we have the honour of continuing our paper here, we shall endeavour to do the same for loaded ships under certain conditions.

Mr. Wigham Richardson: My Lord, something has been said about the way in which this curve has been calculated. I stated that it had been taken according to Sir William Fairbairn's method, which, I believe, is nearly accurate, and I think it must be familiar to many members of the Institution. At any rate, I do not know of any other where it is so carefully and so much in detail gone into. He attempts to give what he considers a fair value for every portion of a ship's structure. I shall not, my Lord and gentlemen, take any notice of the facetious remarks of Mr. Martell, which, I presume, were intended more for our amusement than anything else. But as regards what Mr.

John says, the very point that he raised, namely as to the numerals being wrong, and that I must not assume that the scantlings went *pari passu* with the numerals, *that* was the very point that puzzled me, and perhaps all of us, but the answer is *this curve*. These are the sections taken from ships drawn down according to Lloyd's Rules, and varying in the lengths which they give in their Rules; that is to say, here is a ship of very extreme proportions, but they are proportions recognised in Lloyd's Rules. They have been calculated in detail by Mr. Ruthven, the head of our staff, son of the well-known introducer of hydraulic propulsion, and here is the result given.

Mr. H. J. Cornish: My Lord, may I point out that three of these five ships could not be built under Lloyd's Rules. They are of such extreme proportions. You could not class a ship 400 ft. long, 34 ft. beam, by 25 ft. moulded depth. Lloyd's would only accept two of these at all.

Mr. Richardson: I think that gentleman is incorrect.

Mr. Martell: He is quite right.

Mr. Richardson: I beg you will remember that this has been drawn up carefully and taken from Lloyd's Rules, and, speaking from memory, I think that Lloyd's Rules go up to such extreme proportions as 15 or 16 depths.

Mr. Cornish: Ships of that depth must be built under the "Three-deck" Rules.

Mr. Richardson: These are under the "Three-deck" Rules.

Mr. Cornish: Then you must take the depth for proportions to the middle deck. The second ship will be nearly 16 depths in length, and the other three might be 20 depths and more in length.

Mr. Martell: That is quite right.

Mr. Richardson: I put it to you here again that you have here a depth of 25 ft. moulded.

Mr. Martell: That is to the upper deck.

Mr. Richardson: Twenty-five ft. moulded; take sixteen times that and you get 400 ft.

Mr. Martell: You would have to take at least 7 ft. off that, which would cause the ship to be 22 depths in length.

Mr. Richardson: You get 400 ft., but that is, after all, not the important part of the paper. Mr. John spoke about the paper being full of fallacies, but I request the members of the Institution again to notice that what I particularly wished to do was not so much to go into an exact determination of abstract principles, but to draw the attention of the shipbuilding world to the fact that three important societies have three different bases for calculating strains, and I think that I did not express an exact opinion on any of them.

Mr. W. E. Smith: My Lord, I wish to make one or two remarks in reply to the criticisms on my paper; and first I will take the remark made by Mr. White. He is perfectly correct in supposing that I did not advocate in my paper the application of this method to each particular ship. By way of comparison, the ordinary method is just as good, no doubt, as this method is. I simply wanted, as he supposed, to make a quantitative estimate of what was left out of account in omitting to include this feature of wave stratification in making calculations of bending moments in a seaway. With respect to the difficulty he supposes of applying a strain-indicator, that was dealt with almost entirely by Mr. John. I must say, as far as I have been able to think about the matter, there can be no difficulty in applying the instrument. The difficulty I see is in making the

instrument. Mr. Stromeyer made an instrument which bade fair to do work of this sort, and when I wrote the paper I put in the remark about that instrument, knowing that Mr. Denny—who, unfortunately, is away on account of illness—had purchased one of them with the view of applying it in the way suggested by the paper, and I wanted to get information from him as to whether what had been expected of the strain indicator was realisable. It seems to me that if we are ever able to make such an instrument, we can easily apply it to any part of the structure we like. We can apply it, for instance, to an iron deck, notwithstanding that there may be a wooden deck over it, for if we take out one strake of wood—the strake next the water way, or something of the sort—it would make no difference to the structural strength of the ship. So long as the instrument is one which does its work satisfactorily the application of the instrument will give, I am sure, no difficulty. I think Mr. White will agree with me on this point. The only other point I would wish to make remark about is that raised by Mr. Maginnis. I perfectly agree with Mr. Maginnis's criticism in the abstract, but do not consider it applies to my paper at all. I have endeavoured in my paper to put everything down with an entire absence of a perplexing X or Y, or anything of that sort. If Mr. Maginnis can point out any abstruse formula or anything that he, as a practical man, not having much time, has difficulty in applying, I shall be very pleased to help him out of it. I can assure Mr. Maginnis that the application of these scientific principles is the great plague of my life, and the great plague of all our lives, and no one is more anxious than myself to make the application as easy as the case admits of. The only way it seems to me for a person like Mr. Maginnis to get out of the difficulty and to be able readily to apply anything of this sort, is either to go himself or to send somebody else from his firm to such a place as the Royal Naval College, where instructions are given in the most facile methods of applying all the formulæ which we are all anxious to make as simple as possible, so as to get to the bottom of all these intricate questions. Those are all the remarks I have to make. I am very much obliged to the meeting for the favourable criticism they have given to my paper. It has caused me many hours of work to write, and I have endeavoured to put the results in just that form that Mr. Maginnis desires.

The President: Gentlemen, in conveying your thanks to the writers of the respective papers, I will only add just these two or three words. I think Mr. White did very great service to-night in recalling the attention of this meeting to the most valuable paper which Mr. Smith read to us, and I am quite sure Mr. Smith must feel gratified himself by the encomiums that have been passed upon it when he bears in mind the high sources whence those encomiums have come. Mr. Smith—I think as President I am entitled to say this—is a striking illustration of the value of that scientific education which is given at the institution in which we all take so deep an interest at Greenwich. And I may also add, I am sure I am not exaggerating Mr. Smith's merits when I say that it is a gratification to all of us to feel that our young naval architects are under such able guidance, and in process under that guidance of being led to that high position of eminence which he has obtained so young, and which we hope many of his pupils will obtain after him. I beg to return our thanks to the writers of these two papers.

THE ADVANTAGES OF INCREASED PROPORTION OF BEAM TO LENGTH IN STEAMSHIPS.

By J. H. Biles, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

My object in reading this paper before this Institution is to give some of the advantages which have actually been obtained by adopting increased proportions of beam to length in some steamers which have been built recently by Messrs. J. & G. Thomson, of Glasgow. This firm have for some time past made it a rule, in tendering for any ship of large size or high speed, to suggest an alternative design to that proposed by the shipowner, and the modification has invariably taken the form of increased proportion of beam to length. In some cases the proposed modifications have been adopted, and the actual results of some of these ships are now laid before this Institution.

It will be remembered that it was the late Mr. Froude, in his paper on "Comparative Resistances of some Long Merchant Ships," who first brought prominently into notice the fact that it was possible by increasing the beam and by fining the ends to get a form which at high speeds would have considerably less resistance than the ordinary merchant ship form with ten beams in the length. The alternatives which he gave were, however, exceedingly fine-ended, and they did not appear to possess very much advantage at moderate speeds. Consequently they have not been much adopted, but since the reading of that paper there has been less tendency to look upon an increase of beam as likely to reduce speed, though there has been not much notice taken of the fact that this increase should be associated with an increase of fineness in the ends. It is necessary to note therefore in all that follows, that whenever an increase in the proportion of beam to length is spoken of, it is assumed that it is accompanied by an increase of fineness in the form.

Increased proportion of beam to length gives:

Increased speed. It is very difficult to deduce really reliable information as to the relative merits of different forms of ships from the results of their steam trials. The efficiency of the means of propulsion is a large element in the gross result, and it is not possible in the present state of our knowledge to take a proper account of this efficiency. The only possible method of comparison is to choose ships somewhat similar in size,

speed, and elements of propeller, and from the results of their steam trials deduce the relative efficiencies of the form by means of the Admiralty formula, or some similar one, which we know for small variations cannot be very far wrong. The comparative results cannot, however, be considered as accurate, but will give some rough guide to the relative merits of different forms.

The following are the particulars of two vessels, built by Messrs. J. & G. Thomson. They are mail steamers carrying cargo of such a nature that internal capacity is of more importance than weight-carrying :—

	Length.	Breadth.	Draft on Trial.	Block Co-efficient.	Prismatic Co-efficient.	Kirk's Angle	I.H.P.				Admiralty Dispt. Constants.			
							15.	14.	13.	12 knots.	15.	14.	13.	12.
A	390	42	18·2	·646	·694	9°20′	5130	3390	2280	1660	216	256	301	324
B	375	45	20·3	·598	·651	9°5	3940	2860	2280	1810	280	311	315	316

These two cases I submit to this Institution as a direct proof that the broad but fine-ended steamship B is a more economical form to drive than the larger, narrower, and fuller-ended form A.

As may be seen by the co-efficients, they are both fine ships, and therefore the comparison is, in my opinion, a fair and a typical one. The displacement on trial was 5,500 tons in A and 5,900 in B. The particulars of the propellers are :—

A.	Diameter.	Pitch.	Actual Surface.	Revolutions at 15 Knots.	Slip per cent.
A	18·6	28·6	102	63·6	15·8
B	18·0	28·0	112	63·4	14·5

These propellers are both of comparatively small diameter for the power developed, but it is not necessary to state here why they were made so small. They are not very dissimilar, except in the surface, which is 10 per cent. greater in the case of B than A. As well as I have been able to determine from the data at my disposal, it appears that within certain limits a considerable alteration may be made in the surface of a propeller, and the result obtained may not be much altered in smooth water speeds. The forms of the two propellers are very similar. Whatever advantage there is in diameter, will most probably be in favour of the propeller of A. Whatever difference there may be, cannot be great compared with the whole difference in the results. The only other difference which may have affected the result is the state of the bottom. A had been

launched five months before trial, and her bottom had not been painted again below the 10 feet W.L. B had been docked three weeks before her trial. This, however, could not have caused much loss of speed, as we have always found that vessels which have been lying in Messrs. Thomson's wet dock, when they have gone into dry dock, have been practically as clean as when launched. Further, we have tried several vessels on the Clyde before docking, and they have been tried at Stokes Bay immediately after docking, and generally with very little difference in the result, the greatest discrepancy amounting to 6½ per cent.

The Admiralty constants show that the vessel B was nearly 30 per cent. better than A. In further confirmation of the superiority of the broader ships, the following two cases are given :—

	Length.	Breadth.	Moulded Draught.	Block Co-efficient.	Prismatic Co-efficient.	Kirk's Angle.	I.H.P. at 14 Knots.	Admiralty Constant at 14 Knots.
C	365	43	17·1	·588	·656	8·37	2,910	257
D	365	45½	17·4	·513	·624	8·41	2,740	282

In these cases there is a difference of 10 per cent. in favour of the broader ship. These vessels had lain about the same time in the water after launching, and had not been docked previous to their trials. Their propellers are very similar, being as follows:—

	Diameter.	Pitch.	Actual Surface.	Revolutions at 14 Knots.
C	17·6	25·6	95	61·8
D	17·6	25·6	90	61·5

Though there may be some reason for doubting the whole of the advantage in the case of A and B, there appears to be quite sufficient to warrant one in saying that increased proportion of beam to length gives greater speed when it is combined with finer ends.

The displacements of A and B at 24 feet moulded draught are 7,690 tons and 7,240 tons respectively. Assuming that the total weight of hull and machinery are the same in the two cases, it will be necessary to increase the dimensions of B to length 386 and the breadth to 46·4, in order that the two ships may carry the same total dead-weight of coals and cargo on commencing a voyage. From data obtained from ships built to Lloyd's three-deck rules, it appears that the vessel B enlarged would not exceed A in the weight of hull more than 1 per cent., the depth being the same.

The difference in first cost in the two vessels, A and B, as enlarged, would not be more than 5 per cent., but the difference in horse-power, and therefore in coal consumption, for the same speed would be at least 10 per cent. Assuming that a vessel of this class costs £90,000, and that the machinery costs about £40,000, the extra first cost on the vessel will be about £4,500, and the decreased first cost on the machinery about £4,000, or a total increased first cost of £500 for a decreased consumption of fuel of 10 per cent., which is equal to at least £1,000 per annum. This is assuming that the same total weight of coals and cargo be carried as in A, but obviously a decreased coal consumption means an increased cargo-carrying on the same draught. The actual dimensions which would give the same cargo-carrying in the two types, after providing sufficient coal for the voyage, must depend upon the length of the voyage, so that each case must be decided according to its special circumstances.

Increased proportion of beam to length allows of *increased depth for the same amount of initial stability*. To show the difference in initial stability due to the position of the metacentre, the curves in Fig. 1 (Plate X.) are referred to. From these it will be seen that for the same position of C G, the metacentric height in the light condition may be increased by about 1·1 ft. in passing from A to B, and ·6 ft., in passing from C to D. To a ship which has a negative metacentric height of over 2 ft. in the light condition, as several large ships trading to London have, this addition would be of the greatest value in the successful working of the ship in dock. If, however, the stability in the narrow type has been found to be sufficient, a considerable addition may be made to the depth of the vessel when the proportion of beam to length is increased. The advantages due to this extra depth are: --

Increased strength due to the increase in the depth of the virtual girder which the ship forms;

Increased finite stability, on account of greater freeboard;

Increased surplus buoyancy, on account of greater freeboard;

Increased internal capacity for cargo.

If the cargoes are generally dead-weight, this increased capacity will be useless for cargo purposes, and will be costly for maintenance, on account of the extra tonnage dues which must be paid. If, on the other hand, the cargoes are light in density, the extra capacity will be of value, and if the increase of proportion of beam to length be made with the increase of depth, a point must be reached where limitation of amount of cargo which can be carried will be the same as in a dead-weight trade, viz., draught of water and freeboard, and in that way all cargoes may be reduced to the same basis as a dead-weight one. One consequence of this will be, that the saving of weight which follows the adoption of steel will be as remunerative in a ship carrying light as in one carrying heavy goods.

It may be remarked, however, that generally an increase of depth is not a necessity of increased proportion of beam to length, as few ships have too much initial stability, but

that if an increase of beam gives too much initial stability it is easy to reduce this stability by increasing the depth of the ship.

Increased proportion of beam to length gives of itself *increased strength to the ship longitudinally* by

(1) Generally reducing the absolute length of the ship, and thus reducing the straining action due to length.

(2) Increasing the breadth of the decks and the bottom amidships at which points the ship receives the greatest straining action. This is most increased in the decks, as the openings in the deck are not increased with the beam and consequently the increased breadth is put in where the structure is not cut. For instance, in A the upper deck, less machinery openings, is 26 feet wide amidships; in B it is 29 feet, or 11 per cent. more, whilst the beam is only 7 per cent. more.

The section of A and B to Lloyd's scantlings for three-deck ships built of steel gives a stress of 5·9 tons and 4·8 tons respectively per square inch on the upper deck, on the assumption that

$$\text{Bending moment} = \frac{\text{Displacement} \times \text{Length.}}{30}$$

Increased proportion of beam to length gives of itself *increased range and amount of stability.* The curves of stability of A and B are given in Fig. 2 (Plate X.), on the assumption that the moulded depth and the vertical position of the centre of gravity are the same in both cases.

Fig. 3 (Plate X.) shows the longitudinal distribution of buoyancy in the vessel A and B at the mean draught on trial. Fig. 4 (Plate X.) shows similar curves for C and D. Figs. 3 and 4 show similar curves for A and B and for C and D, but at the deep load line assumed, the same in all cases. Fig. 3 shows similar curves A and B enlarged to A's displacement. Fig. 4 shows similar curves for D and for C enlarged to D's displacement. Figs. 5 and 6 (Plate XI.) give similar curves showing the longitudinal distribution of the internal capacity of the ships.

These have been included, as they afford a ready means of comparing the forms of the ships, and it is thought that they may be interesting.

NOTE.—The metacentric diagram of steamer E is given to show that a very considerable increase may be made in the beam without raising the metacentre much, provided that the ends of the ship be fined.

DISCUSSION.

Mr. W. Richardson: Might I just ask a question for information as to this very interesting paper? If Mr. Biles will kindly refer to page 155 he will see there are 15 knots to steamers A and B, the one with over 5,000 and the other with under 4,000 horse-power. In the next table below that the revolutions at the same speeds seem to be almost precisely the same. The question I wish to ask for information is, were the cylinders of one ship much larger than in the other? The indicated horse-power in the one is very much larger than that in the other, and the revolutions are the same. Therefore I want to know, were the cylinders greater to give that indicated horse-power, or was it got in some other way?

Mr. James Hamilton, Jun.: At the very threshold of this paper the author seems to find it necessary to disclaim having been led away by the comparatives resistance of long ships made by Mr. Froude. He seems to find it necessary to disclaim having made the mistake that many of our friends have made in working from those experiments. I think the experiments (it is no fault of Mr. Froude, it is the fault of the people who fall into the error) gave the length and breadth of ships, but I think there was no co-efficient or any means by which one could readily discover what the forms of those ships were. The forms of those ships were excessively fine that Mr. Froude tested, and I believe many people have fallen into the error of having increased the proportion of breadth to length of ship without having taken care to find the ends. I think Mr. Biles is to be congratulated that he has not fallen into this error—that he has erred on the safe side; because I hold that this ship B, and in the other example the broad ship D, as compared with C, are, in the true sense of the word, finer ships than the other ships are—considerably finer. Of course, Mr. Biles may not agree with me in that, and there may be many gentlemen who may not agree with me, but it depends altogether upon the measure that we are to apply to fineness and fulness of a ship. Model experiments alone, I believe, will determine properly what is the proper measure of fineness and fulness, and, I suppose, it comes to this, that the ship that goes with the least resistance is the finer ship, and the ship that has most resistance is the full ship, that is, in dealing with different proportions of length to breadth and fineness of form. In the absence of model experiments, I have been in the habit of taking the breadth plus the draught of water multiplied by a co-efficient—the co-efficient of fineness; and I find that this measure agrees with those four vessels given by Mr. Biles, if taken at what might be regarded as the full speed of those ships, something like 14 or 15 knots, very closely, and measures the fineness or the fulness of those ships; in fact, the resistance I think follows within 7 or 8 per cent. of that measure. Do not misunderstand me. I do not put this forward as a measure to be put in competition with the Admiralty formula, because the question of speed does not come into it at all. It might take the place of $D^{\frac{2}{3}}$ perhaps, or the area of the midship section, but it is simply a measure for dealing with ships when they are put into the shape that Mr. Biles has put them into, when the ships are about the same lengths and when the speeds and displacements exactly correspond. Now, I have put these vessels into that shape, and I find that A would be represented by 38·89 and B by 39. Therefore, I think B is to all intents and purposes the same as A, and we would expect that she would take the same horse-power to drive her, but she did take less horse-power. In the same way C is represented by 35·3, and D by 32·26, so that D, measured in that way, is a finer ship than C. By these figures, taken as the rule of three, the displacement of A being 5,130, the horse-power would be 5,140, whereas the actual horse-power, I think, was 3,940. I do not agree with Mr. Biles, and I think he has some

hesitation himself in saying that a ship that has been lying in the Clyde for five months would have no greater resistance than one that has been lying only three weeks. In the only direct experiment that we had occasion to make, we found that after having taken a vessel upon a slip and painted her bottom, we really got 20 per cent. more speed. I am quite sure there would be a very considerable difference in the resistance between a ship that had been lying in the Clyde five months and one that had been lying only three weeks. At the same time, even when we have made this allowance, it seems there is a fair balance in favour of the broad ship when she is fined to the same extent that this vessel has been. I do not know that the four examples, though they are very valuable, are sufficient to enable us to state accurately that the resistances of broad ships and fine ships would be quite as represented in this paper, but I think the greatest credit is due to Messrs. Thomson for having put the results of four such large ships as that before us. Few firms can put such an array of data as that of four large steamers before us, and I think they deserve the greatest credit, and Mr. Biles also for having worked it out.

Mr. J. Inglis, Jun.: My Lord, I think the title of this paper might have been better worded; it might have been, "Upon the Disadvantages of Increased Proportion of Length to Beam in Steamships," for this reason, that the increased beam can scarcely be called an innovation so much as a return to the tradition of our elders which we have perhaps discarded a little too readily. I do not think it is necessary to go back to the book of Genesis for the specification of a very old vessel which was perhaps not formed so much for speed as for seaworthy qualities; but if we only go back as far as 1866, to the Transactions of this Institution for that year, we find there that it was gravely argued whether a vessel was not made unseaworthy by being made eight beams in length. And a very distinguished member of this Institution, Sir Edward Reed, I remember, deprecated the idea that it should go forth to the world as the opinion of this Institution that the *London* foundered because she was excessively long, because he, for his part, did not think she was an excessively long vessel. The *London* was under seven and a half beams, so that the advantage of beam seems to have been recognised even in those days as a good thing for seaworthiness. So far as regards the advantage in some cases of an increase of beam for speed, my own observation quite corroborates what Mr. Biles says, only I think that the dimensions of an ordinary merchant ship have not been ten beams in length for so very long as to give that proportion the authority of antiquity. It is quite within the recollection of all of us when a vessel of ten beams was considered almost a monstrosity in length. Mr. Biles has certainly done very good service in pointing out that even more moderate lengths might be too long for purposes of speed, and that the increase of beam of a vessel of even less than eight beams in length might be attended with advantage. I remember in 1865 we were asked to construct two small paddle steamers for the river Clyde, and we made an experiment then which was very interesting, and the result was the exact opposite of what all the wise people in the neighbourhood predicted. We built one vessel to 195 feet in length by 22 feet in breadth. The other vessel was 190 feet long by 23 feet in breadth, that is to say, 5 feet shorter and a foot broader; the displacement was the same; the engines, boilers, paddle wheels, and everything exactly the same, and the shorter vessel went fully a knot faster than the longer one. Of course her draught of water was less. But the displacement was the same, and the advantage in speed was what I have said. Another point is this. Mr. Biles, of course, knows perfectly well—as well as anybody, and I do not think he has laid sufficient stress upon it—that, in talking of the advantage of beam and fine ends, he is speaking of those fast mail steamers that Messrs. Thomson have built so many of. I do not think he would claim so much advantage for his proposal in the case of merchant vessels to carry heavy cargoes at low speeds;

in fact, if you look at the tables of the vessels A and B, as far down as 12 knots, the long vessel has rather the advantage, and I think it certain that at speeds such as 8 or 9 knots the long vessel would have very much the advantage. If I am not mistaken, that was pointed out by Mr. Froude in some of his papers—that, on account of the skin friction being the most important element in the resistance at low speeds, the effect of form was not so marked in the results as the effect of the diminution of the amount of skin, which perhaps could be made less in a long parallel-sided vessel. The comparisons I have made at any rate lead me to the conclusion that for the heavy weight carrying ship of a low speed, a broad vessel with fine ends is not the best form.

Mr. R. E. Froude: My Lord and gentlemen, I did not quite understand Mr. Hamilton's reference to my father's paper on the comparative resistance of long and short ships. I understood Mr. Hamilton to say that there was nothing in that paper to indicate the peculiarity of form which was recommended to accompany the increase of breadth. I understood him to say that increase of breadth was recommended, but that increase of fineness of ends was not laid stress upon. I therefore wish to point out that the lines of the forms which were compared were on the diagrams accompanying the paper and printed in the Transactions. Perhaps I misunderstood Mr. Hamilton's meaning, but I think the paper sufficiently made clear the characteristics and differences of form upon which the gain in the performance depended. In reference to the comparison between the ships A and B, Mr. Biles gave reasons for supposing that the gain in the performance in the ship B was not due, or was not by any means entirely due, to any excessive resistance due to friction of skin in the form A. Mr. Hamilton contests this, and says that he thinks the ship certainly should have been foul if she remained in the water so long. There certainly seems some reason in that, but there is in the internal evidence afforded by the results ground for supposing that the friction was not excessive in the ship A, in the fact that at the lower speed the performance of the ship A is the best of the two, and as skin friction is most important of all at low speeds I think that certainly the ship cannot have had any very great excess of skin friction. I certainly feel myself personally very grateful to Mr. Biles and Messrs. Thomson for doing so much to introduce the principle which was advocated by my father in that paper into practice in the Mercantile Marine. I should wish, however, to point out one qualification in particular which my father introduced in advocating that principle, which was this. Mr. Biles is here dealing with results of trials in smooth water. Now, it is certainly the case that a diminution of the fulness of the ends and concentrating the displacement in the middle of the ship and removing it from the end, is certainly likely to make the ship pitch, and it is not only objectionable on that ground, but the performance of such a ship in a seaway would, from that reason, be comparatively less favourable than in still water, because the pitching must certainly rather tend to increase the resistance. So that it is probable the gain in the performance which we find in these trials to be realised by ships with fine ends in still water is greater than they would evince in practically working at sea.

Mr. W. John: My Lord and gentlemen, when we compare the differences between A and B ships and C and D ships, I would like to draw attention to this one fact, that in A and B the propellers are almost identically alike, and there is not such a very large difference between the horse-power for the particular speeds, although the increase of beam is nearly as great in the one case as in the other. But in the comparison between A and B there is a difference of 6 inches in the diameter of the propeller, 6 inches in the pitch, and while the diameter and pitch are reduced, the surface is increased by 10 feet. I do not know whether there was any difference (perhaps Mr. Biles can tell me as I go on) in the immersion of the screw in those two ships. Were they at about the same draught?

Mr. J. H. Biles: They were both well immersed, I think. One was immersed about 18 inches, and the other probably 22 or 24: they were both well immersed. For all practical purposes they were practically the same in immersion, I think.

Mr. John: I think it is quite possible. While I entirely agree with Mr. Biles' idea, following Mr. Froude's ideas, and following ideas that prevailed even before Mr. Froude demonstrated it, that, by increasing the beam of ships and fining the ends, you can get better speed with the same power, I have had a little experience lately which induces me to think that there may be even more in those little differences in the propellers of those two ships A and B than even in the foulness of the bottom or in the difference of skin friction. I will give you a couple of examples with regard to two sister ships that I have had on trial comparatively recently. For experimental purposes the screws and the spare screws were made slightly different. There was not more than a difference of 6 inches in the diameter, and very little more difference in the pitch than that. We put on our favourite screw at first, because we thought we should develop the power and get the speed easier. And we did not within about a quarter of a knot. Then we put on the coarser screw, and we got scarcely such a good result. We just tipped the ship up, altered the trim a foot, which would bring the screw 6 inches more out of water, and I must say it was purely to my mind a toss up whether we should improve—I thought we should improve matters, but I will defy anyone to prove it beforehand. The result was that we got nearly half a knot more speed, the ship's bottom exactly the same and the ship exactly the same. We had almost an identical experience with the sister ship within a couple of months after that. Therefore, I am sorry that there is so much difference between the screws of those two ships, because I think it is quite possible that the difference may arise from that quite as much as it might from either of the other causes assigned here. There is only one other remark I would make. I think, now we have started increasing the beams of our ships and fining the ends, we shall probably go on until we are brought up by the one simple question of stability—that we shall go on increasing the beams, we shall get better performances, until we get the ships so stiff that they will begin to roll and become uneasy ships at sea, and then we shall begin a retrograde movement, and the sooner we can realise that position and steady our oscillations and not go too far in the one direction or the other, the better it will be for the Mercantile Marine.

Mr. E. Withy: My Lord, I should like to point out a difference between the two ships A and B. If you refer to the first table on page 155 there is a difference of draught of 2·1 feet. I should like to ask Mr. Biles if he can give us the figures of the displacement of those two ships at those respective draughts. It will be a valuable addition to the paper, I think. I presume that the coefficients are taken at the draughts given. Perhaps he could confirm that. Then I should like to point out that with regard to the indicated horse-power, it is identical for both ships at 13 knots and is more for the broad ship at 12 knots. Perhaps Mr. Biles will give us some explanation of that, or some theory with regard to it.

Mr. J. H. Biles: My Lord, in replying to these questions I will take them in order, if you will allow me. Mr. Richardson has asked whether the engines were the same. The volume of the cylinders was about the same in the two cases, but in the case of B, that is a three-cylinder engine, and A is a two-cylinder engine. The capacity of the boilers is a little more in the case of A than in the case of B, but the pressure at which the boilers were working was considerably more in the case of A than in the case of B—about 15 lbs. mean pressure. With respect to Mr. Hamilton's remarks about the measure of the fineness of a ship, I have had the pleasure of reading a paper he read at an institution in Scotland on a similar subject to this, and in which he explained that; but I have not been able

thoroughly to appreciate the value of this co-efficient. I do not care to say anything more about it at present, because I hope, before Mr. Hamilton's paper comes on to-morrow night, to be able to get a little appreciation of it, and also to be able then perhaps to say whether, in my humble opinion, the thing is of as much value as Mr. Hamilton thinks it is. I have some data worked up in connection with it, but I have never been able to properly digest it in order to give anything like a definite opinion on the question. Perhaps, my Lord, you will allow me to ask Mr. Hamilton a question?

The PRESIDENT: By all means.

Mr. BILES: He says he tried a vessel on the Clyde. He tried it before docking and after docking, and got a difference of 20 per cent. in the speed. I think there is a little error in that. If there is not, I should like to know what the absolute speed was.

Mr. HAMILTON: Did I say 20 per cent?

Mr. BILES: Yes.

Mr. HAMILTON: The actual speed before docking was 10½, and we got half a knot more. Of course that is a very different thing. I withdraw the 20 per cent.

Mr. BILES: That is about 5 per cent. on the speed, which probably means about 15 per cent. on the horse-power. Assuming Mr. Hamilton's figure of 15 per cent., instead of taking the figure I gave, 6½, still leaves the broad ship 15 per cent. to the good; 30 per cent., as I have shown, is probably considerably on the wrong side; that would still leave it 15 per cent. better. I would make it compare more favourably with the cases C and D, as given where the difference is given as 10 per cent. I do not wish to claim any credit for proposing this increase of beam. As Mr. Inglis says, you want to go back as far as the Book of Genesis before we get to the person who deserves the real credit for the particular proportions. But though I do not wish to bring in the Book of Genesis I did not intend this paper to be a Book of Revelations. Mr. Inglis has said with regard to the question of narrow ships, that the narrow ship is the better ship for slow speeds. Mr. Hamilton—I am sorry to drag him into the discussion again, but he has worked up some very valuable data in connection with that, and I must leave him to reply to that remark in his paper to-morrow night, and I think he will be able to lay some data before Mr. Inglis for his consideration which may alter his opinion. I am very much obliged to Mr. Froude for the commendation he gave with respect to the paper. With regard to Mr. John's criticism, I thoroughly appreciate the difficulty of comparing results of trial trips, as I have mentioned in the paper. I have a lot more results than these, which might have been brought out, but they did not appear to fit in very well. I will not say they do not fit in with the theory, but there are certain things that might be taken exception to, such as Mr. John has suggested; the difference of propeller, for instance. The broad ship generally shows a better result than the narrow ship, yet there are differences in the propeller which may or may not have accounted for it. To give an instance alongside Mr. John's, I know of two ships of the same dimensions, practically the same form; they both have the same diameter and the same pitch of propeller, but one has a surface of 72 feet and the other of 91 feet, and those vessels are practically the same in speed,—not only in speed over the measured mile, but in speed over long ocean voyages taken over several years now. That seems to be rather a remarkable result. Anyone who has considered this question must know there are many peculiarities in connection with this question of the screw propeller, but the only thing I could do (and I submit it to the consideration of this Institution) was to bring the results, which were as

nearly as possible alike, in order that this uncertain element could be as nearly as possible eliminated. It may be that even that small element does introduce a large difference, but I do not think that the small difference in the propellers is so likely to produce a large difference as the large difference in the propellers would be. The displacement of the two ships on trial, I think, Mr. Withy will find is given in the paper. One is 5,500 on trial, that is the ship A, the narrow ship; and the ship B has a little more displacement, 5,900: that is on page 155. The difference of the displacement, of course, is taken account of approximately in the Admiralty formula. The Admiralty displacement constant is shown there, so that the difference given by the constant is 216 instead of 280. I think, gentlemen, that is all I have to say.

The President: I am sure you will let me thank Mr. Biles for his excellent paper, which exhibits all the care that he invariably takes in preparing his papers. Nothing strikes me so much in the proceedings of this Institution as the extreme care and research which our young men show in preparing their papers.

ON A METHOD OF REDUCING THE ROLLING OF SHIPS AT SEA.

By P. Watts, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The object of this paper is to draw attention to a method of reducing the rolling of ships at sea.

When the *Inflexible* was in course of being designed, it was found necessary to accept great metacentric height in the normal condition, in order that when the ends were injured or partially destroyed the vessel might still have sufficient stability. Instead of a metacentric height of about 3 ft., which would otherwise have been deemed sufficient, a metacentric height of nearly 8 ft. had to be adopted. This gave a metacentric period of 10·7 seconds; and it was recognised that this shortness of period would necessarily tend to produce quick and deep rolling under conditions of sea likely to be often met with, although the great beam and flat floor of the ship were likely to favour steadiness. Mr. Froude's investigations and experience with the earlier ironclads had shown that this would be the case. Some of these ironclads had metacentric heights of 6 ft. and periods of about 10 seconds, and they were well known to be bad rollers, while on the other hand the more modern vessels, which had less metacentric heights and much longer periods, had proved to be steady and comfortable ships.

The consideration arose whether it would be possible by some device either to considerably reduce the metacentric height in the normal condition for ordinary sea-going purposes, and thus reduce the tendency to roll; or to increase the resistance to rolling possessed by the ship considerably beyond that possessed by ships under ordinary circumstances; or to effect a combination of these results in a greater or less degree.

Mr. Froude had shown that the resistance to rolling possessed by ships might be very greatly increased by the use of bilge-keels of sufficient depth. Such appendages, however, to be effective in a ship possessing great moment of inertia, must be of very large dimensions, and therefore much exposed and liable to injury, besides offering resistance to the motion of the ship through the water; moreover, in the case of the *Inflexible* the requirement that she should be able to enter existing docks precluded the use of very deep bilge-keels.

Several devices were discussed; that preferred and adopted is known as "water-chambers." The latter were compartments across the ship into which free water might be admitted when it was required to reduce rolling. Two compartments were originally set apart for this purpose, as shown on Fig. I. (Plate XII)., one forward and the other aft, the one forward being 22 ft. long, and extending from the armour deck to the upper deck, and the one aft 14 ft. long, and extending only from the armour deck to the main deck. But during the completion of the ship, and under the pressure which is always brought to bear to get as much out of each vessel in the way of offensive and defensive power as she can be made to yield, the former chamber, which was by far the larger of the two, was necessarily appropriated for stowage, so that only the after chamber is now available for the purpose originally contemplated.

The notion of making use of a free liquid for quelling oscillations is not new. The method of testing whether an egg is raw or cooked, by making it oscillate in the direction of its largest diameter, and observing whether the oscillations are few or many, will suggest itself as an example of a semi-liquid reducing oscillations. Sir William Thomson has recently used the method for quelling oscillations in his compass. Other instances might be quoted. But I doubt whether it has been used before on such a large scale, or whether free water has ever before been deliberately introduced into a ship for the purpose of reducing rolling. The arrangement, therefore, possesses some novelty.

The water-chamber influences the behaviour of the ship chiefly by modifying the righting force acting upon the ship throughout the roll. The way in which it operates in this respect will be readily understood by regarding the water as doing the reverse to what is done by the men in the process of rolling a ship in still water. The men are timed to run from side to side in advance of the roll, and their weight tends to increase the heel; whereas the water in the chamber, which is arranged to keep time with the ship, necessarily lags behind the roll, as the chamber must become inclined before the water has any tendency to run across by its own weight, and therefore it tends to diminish the heel.

If the weight of the men could be shifted suddenly across the ship, and timed so as to be most effective in producing rolling, then, neglecting friction, the rolling and the motion of the men might be represented thus:—In Fig. II. (Plate XII.), let abscissæ represent time, and ordinates degrees of heel of the ship, port inclinations being measured above the axis, and starboard inclinations below the axis. Let the distance through which the men can travel be represented by the distance between the lines A A and B B. Let the equal distances between $a\ a'$ and the axis on the port side, and $b\ b'$ and the axis on the starboard side, represent the angles of heel at which the ship would rest if the men remained stationary first in the extreme position on the port and then in that on the starboard side. Now, if the men were moved instantaneously from the middle line to the line A A, the ship would start rolling about the inclined position represented by the distance between the short line

a a′ and the axis, and, apart from resistance, would roll to just double this angle of heel. Her motion may be represented by the harmonic curve O P, whose axis is the line *a a′* ; and if at the instant she arrives at the extreme inclination the men were moved across to the line *b b′* on the starboard side, the return roll would be made about the angle of starboard heel measured by the distance between *b b′* and the axis, and may be represented by the harmonic curve P Q, whose axis is *b b′*. If the men be again shifted to the port side when the ship reaches the extreme inclination, the roll may be represented by Q R, obtained in the same way, and so on; the increment of heel at each successive roll being just twice the heel at which the ship would rest if the men remained stationary on one side. As, instead of moving over instantaneously, the men will require a definite time in moving over, their motion must be as indicated by the dotted line, in order to be most effective in increasing the roll. The longer they can remain at the side at the end of each run the more effect will their weight have in rolling the ship, but they must start from any side when the ship heels towards that side, so as to cross the middle line when the heel is a maximum, *i.e.*, they must run *uphill.* The amplitude of each swing would not be so great as represented in Fig. II, and it would be further reduced by the operation of resistance. Now if a ship were rolling from any cause, and it were sought to bring her to rest by running men from side to side, it is obvious that their motion must be the reverse of that represented in Fig. II. They must always be running *downhill* instead of *uphill.* The rolling of the ship and the running of the men might be represented by the drawn and dotted curves in Fig. III. (Plate XII.), supposing the men to have started when the ship reached the extreme heel to port represented by P N.

It will be obvious that the motion of the centre of gravity of the water in the water-chamber of a ship rolling amongst waves, will tend to approximate to that of the men when running as described for the purpose of reducing rolling.

The effect of the motion of the water on the righting force at any instant throughout the roll will be seen from Fig. IV. (Plate XII.). Suppose the water to move so as to be most effective in quelling rolling. In each of the small sections let *g* be the centre of gravity of the water in the chamber, G the centre of gravity of the ship including this water, and B the centre of buoyancy. Let the arrows over the sections represent the direction in which the ship is in each case rolling. In position No. 1 the ship is supposed to have reached the extreme heel to port, and to be on the point of commencing the return roll. Then *g* should have reached the middle line on its way down towards the port side, and the righting force be due to the angle of heel supposing the water to be a fixed weight amidships. In the position No. 2 the ship has performed part of the roll back towards the upright; the water will have moved further down the incline, so that *g* will be some distance from the middle line down on the port side, as shown, and therefore G will also have moved out from the middle line on the port side; hence the righting force will be less than what would be due to the angle of heel if the water were a fixed

weight amidships. In position No. 3 the ship has just reached the upright, and will be moving with the maximum angular velocity; the water will have moved still further down the incline, and *g* will be at a greater distance from the middle line on the port side, and therefore G will have moved further out from the middle line, whereas B will have returned to the middle line, so that the weight of the ship and the upward pressure of the water will form a couple tending to retard the ship's rotation, although she is for the moment in the upright position. In the position No. 4 the ship is heeling over to starboard, and the centre of gravity of the water is returning towards the middle line, but is still on the port side, and the righting force is therefore greater than that due to the angle of heel and a fixed centre of gravity amidships. In the position No. 5 the ship has momentarily come to rest at the end of the starboard roll; and the centre of gravity of the water should have again reached the middle line, and the righting force be neither increased nor diminished by the water-chamber except in so far as it affects the displacement and the vertical position of the centre of gravity. The same process is repeated as the ship rolls back from starboard to port.

Thus, if the shape of the water-chamber and the depth of the water are such that the centre of gravity of the water moves as described, the effect of the water-chamber is to increase the righting force, which opposes the motion, as the ship heels over, thereby reducing the heel; and on the return roll the effect of the water-chamber is to lessen the righting force and cause the ship to move more slowly than she otherwise would, so that she acquires less angular momentum on reaching the upright, and therefore tends to roll less deeply the other way.

In the design of the water-chambers of the *Inflexible* it was the endeavour to arrange them so that the period of the water would be such that its centre of gravity would move as described, under average conditions of sea, &c.

Experiments and observations were made in June last with the view of testing how far the object had in view in the design in this respect had been attained; and also to test other questions, such as what height of water in the chamber would give the greatest quelling effect under various conditions, what was the real value of the water-chamber in reducing rolling at various angles of roll, &c.

It was my duty to proceed to the Mediterranean and conduct these experiments, and I have since received official permission to communicate the results of the observations made to this Institution.

The apparatus used for recording the behaviour of the ship was prepared by Mr. R. E. Froude, at Torquay. It was similar to that made by the late Mr. Froude and used for recording the behaviour of the *Devastation*, which was described by him to this Institution in 1873. In sea-way rolling this apparatus gives a continuous record of the inclination of the ship to the effective wave normal, and also of her inclination to the true

vertical, and therefore also the effective wave slope operating upon her at each instant. Besides this, apparatus was fitted at Malta for continuously recording the motion of the water in the water-chamber. This apparatus was as follows. Floats carried upon rods stiffened by wire stays and hinged to the side of the chamber were fitted across the chamber, as shown in Fig. V. (Plate XII.) Wire cords attached to the rods as at A and led away over pulleys up through the deck, actuated pens upon a recording cylinder. The diagrams taken record the height of each float at each instant, and therefore enable the shape of the surface of the water to be determined at any instant. Time was marked electrically on each of the records by the same clock, so that the several records can be accurately associated in point of time.

I may say with regard to the experiments, that the programme contemplated was not carried out on account of the ship being required for service at Alexandria in connection with the recent Egyptian difficulties. It was intended, if found possible, to roll the ship in still water by running men from side to side, without the water-chamber in use, and with the water-chamber in use and filled to different heights with water, so that *curves of extinction* for the ship could be constructed corresponding to the various conditions of the water-chamber. From these the value of the water-chamber in the several conditions could be obtained, and the resistance to rolling possessed by the ship with and without the water-chamber in use could be at once inferred. But it was not certain that it would be found possible to roll to a sufficient angle so large a ship, having so few clear spaces upon the decks, by running men from side to side; and this failing, it was intended to take records of actual rolling at sea with and without the water-chamber in use, and estimate the effect of the water-chamber from these records. It was, however, recognised that unless these records were taken under exceptionally favourable circumstances as to condition of sea, &c., and included different kinds of seas, they could not be expected to yield the whole of the necessary information with a fair amount of accuracy. In any case it was considered desirable to obtain some actual results of rolling in a sea-way with and without the water-chamber in use, to furnish means for comparing the resistance to rolling experienced in a sea-way, with the resistance to rolling experienced in still water, and also with the view of extending our knowledge of the theory of rolling generally. As I have said, the complete programme contemplated could not be followed.

The ship was rolled in still water without the water-chamber in use, and a roll of rather over 12° from out to out was obtained. Unfortunately this series of experiments was then interrupted, and no opportunity occurred for rolling her with the water-chamber in use; but the results of the experiments without the water-chamber in use show that satisfactory data can probably be obtained in this way. Some sea-way rolling records were afterwards obtained which throw a good deal of light on the value of the water-chamber and matters connected therewith. To these I shall refer presently.

Fig. VI. (Plate XIII.) represents the *curve of extinction* of the ship without the water-chamber in use. The actual curve as obtained from the still-water rolling experiments is the drawn line, and it will be seen that it very nearly coincides with the dotted line, whose differential equation is

$$-\frac{d\Theta}{dn} = \cdot 04\Theta + \cdot 008\Theta^2.$$

On page 174, Table A, the differential equations to the *curves of extinction*, and the periods of several ships are given, and it will be seen that the co-efficients of the equation belonging to the *Inflexible* are almost exactly the same as the co-efficients of the equation belonging to the *Narcissus*. Hence the resistance to rolling offered by the bottom of the *Inflexible* in relation to her stability, is about the same as the resistance to rolling offered by the bottom of the *Narcissus* in relation to her stability. It will also be seen that the *Devastation* offers much greater resistance to rolling than the *Inflexible*. The relative resistance to rolling possessed by these two ships will be inferred from the following:—If the weights of the *Devastation* were lowered so as to reduce her period to that of the *Inflexible*, *i.e.*, from 13·52 seconds to 10·7 seconds; then amongst co-periodic waves having a maximum wave slope (say) of 5 degrees, the *Devastation* would roll 21 degrees, while the *Inflexible* would roll through 30 degrees.* Thus without the water-chamber the *Inflexible* will be a bad roller as compared with the *Devastation*, quite apart from the fact that while the former ship has a short period which will about synchronize with the waves ordinarily met with, the latter ship has a long period, and is unlikely very frequently to fall in with co-periodic waves. The great resistance to rolling possessed by the *Devastation*, as compared with that possessed by the *Inflexible*, is no doubt largely due to the fact that the bilge-keels of the *Devastation* are better placed than those of the *Inflexible*, and also to the projecting armour shelves in the former ship, which offer great resistance to rolling.

The sea-way rolling, of which records were taken, took place on the 16th of June last, when the ship was lying twenty or thirty miles off Alexandria. On this occasion she rolled upwards of thirty degrees from out to out. The wind blew with considerable force from the N.N.W. during the previous night, but fell towards the morning. At 10 a.m. it was said to be of force 5, and it fell off somewhat during the time the observations and records were taken. The sea was irregular, but its general character remained nearly the same while the experiments were being made; it was somewhat less irregular towards the close of this time, and the waves fell off somewhat in size, but series of big waves were frequently met with. The larger waves were of about 10·5 seconds period, and averaged from nine to twelve feet high. The ship was allowed to lie passively in the trough of the sea while the experiments were being made—from about 9 a.m. till 3.15 p.m.

The intention was to commence with the water-chamber empty, and record the

* See Appendix, Note A.

behaviour of the ship for about half an hour, then fill up the chamber by stages, and take records for half an hour in each condition; but the water in the chamber was dashed about with such violence that after the first two or three experiments it was found impossible to keep the hatch open, so that the water admitted at each stage after this could be only very roughly estimated. The water-chamber record enables the quantity to be calculated for the first few stages, but not for the latter ones, as several of the wire cords became carried away, so that the records are of little value either for this purpose or for determining the motion of the centre of gravity. Records were taken, first with the water-chamber empty; then with 40 tons of water in the chamber; then with nearly 55 tons of water; next with 60 to 70 tons; in the next two cases the quantity of water was unknown, but probably it varied in these from 70 to 85 tons and from 85 to 100 tons respectively; as much water was then allowed to run in as would at that draught and trim, and probably the chamber contained 120 tons out of 130 tons which the chamber will hold when full; and, finally, records were again taken with the chamber empty.

For the purpose of estimating the value of the water-chamber, independently of the records taken by the automatic apparatus, Captain Fisher had batten observations taken of the rolling of the ship in each condition of the water-chamber. These observations were of ten minutes' duration in each case. The sizes and character of the waves which happened to be passing at the time were not observed; nor was care taken to ensure the observations commencing and finishing at the right position as to phase. On page 174, Table B, are given the mean and maximum angles rolled through, as obtained from the records taken by the officers of the ship, and from the diagrams recorded by the automatic apparatus over the same ten minutes in each case. There are also given in the last two columns the mean angles rolled through during intervals of about ten minutes' duration, very nearly the same as those chosen by the officers of the ship, but arranged to begin and leave off at about the same point in a phase. The slight differences in the mean angles rolled through as obtained from the angles observed by the officers of the ship, and from the records of the automatic apparatus, are chiefly due to the fact that some small angles rolled through, and intervals during which the ship did not roll at all, which should have been recorded as zero rolls, were omitted from the record by the officers. I compared the angles observed by the officers with those automatically recorded, and found that, with the exceptions named, they agreed with remarkable closeness, the differences being seldom more than a small fraction of a degree. The nature of these differences will be seen on comparing the two columns, giving the maximum inclination in each case. On page 175, Table C, are tabulated the mean and maximum angles rolled through during ten-minute intervals, commencing from every minute between 9.27 a.m. and 10.14 a.m.; and also the mean angles, over intervals of about 10 minutes, and commencing from about the same times, but chosen to suit the phase of rolling. The mean angles will be seen to vary from 10·58° to 15·3°; so that evidently ten-minute intervals were too short for the purpose. It will also be seen that the differences due to the difference of phase may be very

great on such short intervals. Thus, if instead of commencing to record at 9.40 a.m., the officers had commenced a few minutes earlier, the mean angle obtained, instead of being 10·89 degrees, would have been from 13 to 14 degrees; and if they had commenced at 9 hr. 40 m. 18 s. and continued 11 minutes 8 seconds, so as to include complete phases of rolling, the mean angle obtained would have been 12·2°. On the whole, however, the results given on Table B do not generally misrepresent the facts; those marked (*) are probably greater, and those marked (†) are probably less, than would have been recorded if exactly similar waves had passed under the ship in each case.

But such a comparison, in which no account is taken of the character of the waves operating upon the ship, could not be accepted as conclusive, even if the mean angles were taken over much longer intervals.

It was hoped that it would be possible, on analysing the diagrams, to determine in each condition the net forces acting upon the ship, and to divide these into the forces due to wave slope and those due to resistance, and thus to calculate from our diagrams the resistance offered to rolling by the ship with and without the water-chamber in use, and consequently the amount of resistance due to the action of the water-chamber. Several portions of the rolling diagrams have been very carefully analysed with this view, and the results are very instructive as bearing on the theory of rolling; but the resistance due to the water-chamber cannot, apparently, be separated with anything like certainty, with our present knowledge of the subject.

In order to compare the rolling in the several conditions in connection with the size of the waves operating on the vessel at the time, lengths of diagrams in which the conditions as to sea, &c., were nearly the same, were selected and analysed. A complete phase of rolling was taken in each case. The results are given in Table D (page 176). Here there are tabulated the mean wave slope, the mean period of the waves, and the mean function of areas of the waves, the mean absolute inclination of the ship (*i.e.*, her inclination to the true vertical), and the mean relative inclination of the ship (*i.e.*, her inclination to the wave normal), and in the last two columns the mean absolute inclination for 1° of mean wave slope, and the mean relative inclination for 1° of mean wave slope. A comparison of the figures in the last two columns may be regarded as roughly giving the value of the water-chamber on this occasion.* It will be seen on reference to this Table that the water-chamber was most effective when about half full of water. Taking a mean between the results obtained with the water-chamber empty at 10.7 a.m. and at 3.0 p.m. we obtain—

Mean absolute inclination for 1° of mean wave slope = 5·51°.
Mean relative inclination for 1° of mean wave slope = 5·84°.

And taking a mean between the results obtained with from 50 to 55 tons of water in the water-chamber, and from 60 to 70 tons in the chamber, we obtain—

Mean absolute inclination for 1° of mean wave slope = 3·26°.
Mean relative inclination for 1° of mean wave slope = 3·71°.

* See Appendix, Note B.

Hence, by filling the chamber about half full of water the rolling was reduced by about 37·5 per cent. This percentage will be reduced if we include the next experiment, when it was thought there were from 70 tons to 85 tons in the chamber. It will be probably within the truth to say that the water-chamber, when about half full, reduces the mean angle of roll by from 20 to 25 per cent., when the ship is rolling broadside on, through moderate angles, amongst waves of from 9·5 to 10 seconds mean period.

From the results of bilge-keel experiments it appears that the present 2·2 ft. bilge-keels of the ship would require to be increased in depth to 6·5 ft., and in length to about two-thirds the length of the ship, in order that the increase of bilge-keel resistance might have the same quelling effect as the water-chamber, when the ship is broadside on amongst co-periodic waves, and rolling steadily through about 8°.* As the value of bilge-keels varies nearly as the square of the angular velocity, while the value of the water-chamber cannot increase much after moderate angles are reached, it follows that for larger angles of heel the advantage would be with bilge-keels, *i.e.*, less wide keels would suffice to produce the same effect as the water-chamber. On the other hand, for smaller angles the advantage would doubtless be greatly with the water-chamber.

The analyses of the water-chamber records show that on the whole, in this condition of sea, *i.e.*, when the waves nearly synchronize with the ship, the water does not get across from side to side quickly enough as the ship rolls; and it will probably be found that the efficiency of the chamber will be improved generally if changes be made to enable the water to get across more quickly. Fig. VII. (Plate XIII.) is an example of the data obtained from the water-chamber record; it shows the transverse motion of the centre of gravity of the water in the water-chamber with reference to the rolling of the ship during part of the diagram taken with about forty tons of water in the chamber. The curve B B B gives at each instant the inclination of the ship to the true vertical, the curve A A A the inclination of the ship to the wave normal, and the dotted curve C C C the distance of the centre of gravity of the water from the middle line of the ship. It will be seen that the centre of gravity of the water crosses the middle line some time after the ship has reached her extreme inclination to the wave normal, and therefore it does not get across quickly enough. At present there are partial bulkheads across the chamber which retard the passage of the water, and these can be partially or wholly removed.

Some experiments which are in course of being made with a model water-chamber, and some further experiments with one of the ships of the *Inflexible* type, which will shortly be made, will probably settle this and other questions in connection with the matter which yet remain to be decided.

* See Appendix, Note C.

Table A.

Periods and Differential Equations of the Curves of Extinction of some Ships which have been Rolled in Still Water.

Ships' Names, &c.	Period in Seconds.	Differential Equations.
Greyhound, with Bilge keels	7·76	$-\frac{d\Theta}{dn} = \cdot 0198\,\Theta + \cdot 0462\,\Theta^2.$
Greyhound, without Bilge keels	—	$-\frac{d\Theta}{dn} = \cdot 044\,\Theta + \cdot 0032\,\Theta^2.$
Perseus...	7·66	$-\frac{d\Theta}{dn} = \cdot 0187\,\Theta + \cdot 0097\,\Theta^2.$
Inconstant	15·98	$-\frac{d\Theta}{dn} = \cdot 035\,\Theta + \cdot 0051\,\Theta^2.$
Cadmus	9·92	$-\frac{d\Theta}{dn} = \cdot 048\,\Theta + \cdot 0074\,\Theta^2.$
Immortalité	9·9	$-\frac{d\Theta}{dn} = \cdot 0384\,\Theta + \cdot 0126\,\Theta^2.$
Volage	11·58	$-\frac{d\Theta}{dn} = \cdot 028\,\Theta + \cdot 0073\,\Theta^2.$
Narcissus	10·38	$-\frac{d\Theta}{dn} = \cdot 037\,\Theta + \cdot 008\,\Theta^2.$
Devastation	13·52	$-\frac{d\Theta}{dn} = \cdot 072\,\Theta + \cdot 015\,\Theta^2.$
Suitan	17·74	$-\frac{d\Theta}{dn} = \cdot 0267\,\Theta + \cdot 0016\,\Theta^2.$
Inflexible, without water-chamber	10 7	$-\frac{d\Theta}{dn} = \cdot 04\,\Theta + \cdot 008\,\Theta^2.$

Table B.

Mean and Maximum Angles Rolled Through in the Various Conditions of the Water-Chamber as obtained from Batten Observations and from the Records of the Automatic Apparatus on 16th June, 1882.

Condition of Water-Chamber.	Time over which observations were taken.	From Batten Observations.		From Automatic Apparatus.		From Automatic Apparatus, but including complete Phases.	
		Mean Angle.	Maximum Angle.	Mean Angle.	Maximum Angle.	Time of Record taken.	Mean Angle.
		Degrees.	Degrees.	Degrees.	Degrees.	h. m. s. h. m. s.	Degrees.
Empty	9.40 a.m. 9.50 a.m.	10·89†	21·5	10·58	21·6	9 40 18 to 9 51 26	12·20
Do.	10.0 a.m. 10.10 a.m.	13·01	24·5	13·06	24·8	10 0 28 ,, 10 10 35	13·02
40 tons of water in chamber.....	10.42 a.m. 10.52 a.m	14·40*	23·5	14·20	23·3	10 42 11 ,, 10 51 38	14·03
From 50 to 55 tons of water in chamber	11.33 a.m. 11.43 a.m.	10·82	24·0	10·68	23·8	11 33 0 ,, 11 43 10	10·56
Probably from 60 to 70 ,,	12.23 a.m 12.33 a.m.	11·21	20·0	10·84	19·9	12 22 39 ,, 12 33 22	10·68
,, 70 to 85 ,,	1.0 p.m. 1.10 p.m.	8·89	19·0	8·35	19·1	1 0 8 ,, 1 9 35	8·60
,, 85 to 100 ,,	1.37 p.m. 1.47 p.m.	12·10*	23·0	11·23	23·3	1 36 2 ,, 1 47 44	11·46
,, 90 to 120 ,,	1.54 p.m. 2.4 p.m.	8·18†	17·0	8·23	17·0	1 55 4 ,, 2 4 22	8·31
Empty	2.56 p.m. 3 6 p.m.	12·53	23·0	12·55	22·9	2 55 2 ,, 3 4 46	12·65

Table C.

Mean and Maximum Angles Rolled Through, during 10-minute intervals commencing from every minute between 9.27 A.M. and 10.14 A.M.; and also the Mean Angles, over intervals of about 10 minutes, and commencing from about the same times, but chosen so as to suit the Phase of Rolling; on 16th June, 1882.

10-minute Intervals.					Mean Angles.	Maximum Angles.	10-minute Intervals chosen to suit Phase.							Mean Angles.
H.	M.		H.	M.	Degrees.	Degrees.	H.	M.	S.		H.	M.	S.	Degrees.
9	27	to	9	37	13·88	30·4	9	27	0	to	9	36	52	13·90
9	28	,,	9	38	13·33	26·2	9	28	0	,,	9	39	24	14·12
9	29	,,	9	39	14·04	26·2	9	29	17	,,	9	39	57	13·67
9	30	,,	9	40	13·16	24·1	9	30	15	,,	9	40	18	13·15
9	31	,,	9	41	13·14	23·6	9	31	0	,,	9	41	0	13·14
9	32	,,	9	42	13·47	22·4	9	32	13	,,	9	41	44	12·60
9	33	,,	9	43	14·42	22·4	9	33	0	,,	9	43	11	14·34
9	34	,,	9	44	14·52	22·4	9	34	7	,,	9	43	50	14·47
9	35	,,	9	45	13·86	22·4	9	35	18	,,	9	44	25	14·04
9	36	,	9	46	13·39	22·4	9	36	8	,,	9	47	0	13·24
9	37	,,	9	47	13·23	22·4	9	37	16	,,	9	47	0	13·37
9	38	,,	9	48	12·37	22·4	9	38	27	,,	9	48	20	13·03
9	39	,,	9	49	11·41	21·6	9	39	24	,,	9	49	40	11·60
9	40	,,	9	50	10·58	21·6	9	40	18	,,	9	51	26	12·20
9	41	,,	9	51	11·63	28·0	9	41	0	,,	9	51	40	12·04
9	42	,,	9	52	11·98	28·0	9	42	8	,,	9	52	39	12·14
9	43	,,	9	53	11·92	28·0	9	43	11	,,	9	53	0	11·68
9	44	,,	9	54	11·34	28·0	9	44	25	,,	9	54	20	11·40
9	45	,,	9	55	11·63	28·0	9	45	35	,,	9	55	15	11·60
9	46	,,	9	56	11·88	28·0	9	46	30	,,	9	56	24	12·45
9	47	,,	9	57	12·91	28·0	9	47	46	,,	9	57	54	13·56
9	48	,,	9	58	13·75	28·0	9	48	20	,,	9	58	15	14·00
9	49	,,	9	59	14·37	28·0	9	49	21	,,	9	59	6	14·74
9	50	,,	10	0	15·30	28·0	9	50	51	,,	10	0	38	13·91
9	51	,,	10	1	13·23	26·1	9	51	40	,,	10	1	28	12·70
9	52	,,	10	2	12·55	26·1	9	52	39	,,	10	2	5	12·38
9	53	,,	10	3	12·46	26·1	9	53	32	,,	10	3	45	13·44
9	54	,,	10	4	13·65	26·1	9	54	41	,,	10	4	18	13·91
9	55	,,	10	5	14·09	26·1	9	55	15	,,	10	5	28	13·96
9	56	,,	10	6	13·92	26·1	9	56	24	,,	10	6	45	13·13
9	57	,,	10	7	12·72	24·8	9	57	36	,,	10	6	45	12·61
9	58	,,	10	8	12·89	24·8	9	58	15	,,	10	8	12	12·83
9	59	,,	10	9	13·09	24·8	9	59	6	,,	10	9	52	13·19
10	0	,,	10	10	13·06	24·8	10	0	28	,,	10	10	35	13·02
10	1	,,	10	11	13·68	24·8	10	1	28	,,	10	11	59	13·83
10	2	,,	10	12	13·72	24·8	10	2	17	,,	10	13	0	13·52
10	3	,,	10	13	13·30	24·8	10	3	10	,,	10	13	15	12·86
10	4	,,	10	14	12·18	23·8	10	4	18	,,	10	14	9	11·96
10	5	,,	10	15	11·26	23·8	10	5	28	,,	10	15	20	11·51
10	6	,,	10	16	11·60	25·6	10	6	28	,,	10	16	2	11·94
10	7	,,	10	17	12·35	25·6	10	7	7	,,	10	17	41	12·40
10	8	,,	10	18	11·60	25·6	10	8	12	,,	10	18	11	11·44
10	9	,,	10	19	10·73	25·6	10	9	2	,,	10	19	6	10·89
10	10	,,	10	20	11·66	25·6	10	10	17	,,	10	19	59	11·66
10	11	,,	10	21	12·13	25·6	10	11	20	,,	10	20	50	12·20
10	12	,,	10	22	12·57	25·6								
10	13	,,	10	23	12·79	25·6	10	13	0	,,	10	22	26	12·70
10	14	,,	10	24	13·69	25·6	10	14	9	,,	10	24	34	14·77

Note.—The Maximum Angles are the same in each case.

Table D.

Table giving the Details of the Waves, and of the Rolling of the Ship, in the various Conditions of the Water-Chamber, on 16th June, 1882.

Time over which Part Analysed Extends.	Condition of Water-Chamber.	Mean Wave Slope.	Mean Period of Waves.	Approximate Mean Function of Areas of Waves.	Mean Absolute Inclination of Ship.	Mean Relative Inclination of Ship.	Mean Absolute Inclination for 1 degree of Mean Wave Slope.	Mean Relative Inclination for 1 degree of Mean Wave Slope.
h. m. s. h. m. s.		Degrees.	Seconds.	*	Degrees.	Degrees.	Degrees	Degrees.
10.6.58 a.m. to 10.8.37 a.m.	Empty	1·03	9·83	545	5·74	5·93	5·56	5·75
10.49.30 a.m. ,, 10.51.32 a.m.	About 40 tons of Water in Chamber	1·13	9·46	550	4·30	4·97	3·81	4·40
11.36.40 a.m. ,, 11.38.27 a.m.	From 50 to 55 tons ,, ,,	1·16	9·58	550	4·01	4·56	3·46	3·94
12.25.0 p.m. ,, 12.26.32 p.m.	*Probably* from 60 to 70 tons ,,	1·19	9·82	580	3·64	4·15	3·06	3·49
1.8.0 p.m. ,, 1.9.41 p.m.	,, ,, 70 ,, 85 ,, ,,	·86	9·86	500	3·62	4·00	4·21	4·65 †
1.40.40 p.m. ,, 1.42.16 p.m.	,, ,, 85 ,, 100 ,, ,,	1·06	9·84	520	4·33	4·86	4·11	4·60 †
2.2.40 p.m. ,, 2.4.13 p.m.	,, ,, 90 ,, 120 ,, ,,	·77	9·68	385	2·94	3·31	3·81	4·30 †
3.0.8 p.m. ,, 3.1.56 p.m.	Empty	·97	9·36	530	5·27	5·73	5·46	5·94

* The Function of Areas is the Estimated Length of the Wave multiplied by the Mean Slope.

† Possibly the quantity of Water in the Chamber in these Conditions did not vary very much.

APPENDIX.

Note A.

Froude has shown that in co-periodic waves of maximum slope θ_1, the increment of range of roll added per swing is equal to $\frac{\pi}{2}\theta_1$, and that the extinctive effect of the resistance offered by the bottom of the ship to rolling is equal to $a\,\Theta + b\,\Theta^2$, where Θ is the angle of roll, and a and b constants given by the still-water rolling experiments. When steady rolling occurs these must be equal, therefore—

$$\theta_1 = \frac{2}{\pi}\,(a\,\Theta + b\,\Theta^2).$$

Hence, if the *Inflexible* be rolling among uniform co-periodic waves of five degrees maximum slope, the angle of steady roll will be given by the equation—

$$5^\circ = \frac{2}{\pi}\,(a\,\Theta + b\,\Theta^2).$$

$a = \cdot 04$, and $b = \cdot 008$; substituting these values we get—

$$\Theta = 30^\circ.$$

Next suppose the weight of the *Devastation* re-distributed so as to reduce her period to that of the *Inflexible, i.e.*, from 13·52 seconds to 10·7 seconds; and suppose her to be rolling among 10·7 seconds waves of five degrees maximum slope. Then, supposing the transverse radius of gyration

to remain unaltered—the change in period being solely due to change in metacentric height—the differential equation of her curve of extinction will become—

$$-\frac{d\Theta}{dn} = \left(\frac{10{\cdot}7}{13{\cdot}52} \times {\cdot}072\right)\Theta + {\cdot}015\,\Theta^2,$$

and the angle of steady roll will be given by the equation—

$$5^\circ = \frac{2}{\pi}\left({\cdot}057\,\Theta + {\cdot}015\,\Theta^2\right);$$

whence

$$\Theta = 21^\circ.$$

Hence, if the *Devastation* were weighted so as to have the same period as the *Inflexible*, the former ship would only roll 21 degrees among co-periodic waves of 5 degrees maximum slope, while the latter ship would roll 30 degrees among the same waves.

Note B.

The comparison made on page 172 only precisely represents the value of the water-chamber when the ship is rolling steadily amongst co-periodic waves, and on the assumption that her resistance to rolling, both with water in the chamber and also with the chamber empty, varies as the angular speed of her motion.

If the comparison were made with the ship rolling steadily amongst co-periodic waves in one case, and amongst dis-periodic waves in another case, it is evident, apart from considerations of the law of resistance, that the comparison will not truly evaluate the extinctive effect of the water in the chamber; for in the first case the whole of the wave slope at each instant will be employed to balance the effect of resistances; whilst in the second, part only of the wave slope will be so employed, and the remainder will be employed in producing change in the natural period of the ship, in order that it may keep time with the waves amongst which it is steadily oscillating with a roll of constant range.

If the law of resistance depended on the square of the angular speed of the ship the comparison would be inexact, from the fact that the angles of rolling of the ship in any given condition would not be proportional to the slopes of the waves but to the square roots of the slopes, and therefore—

$$\frac{\text{Angle of roll}}{\text{Slope of wave}} \propto \frac{1}{\text{Angle of roll}},$$

instead of being constant, as is assumed.

It appears probable that the resistance offered to rolling by the water in the chamber will be very nearly constant for large angles of roll, and that the accuracy of the comparison will be impaired on this account.

Probably the greatest error in the comparison is introduced by the assumption that the ship is in the condition of *steady rolling* amongst waves of uniform slope and period. The waves were never quite uniform, and the ship scarcely ever approached for a length of time a condition of steady rolling.

The question is, to what extent is the comparison instituted affected by disregarding the considerations which render it doubtful and inexact? Let us consider the rolling of the ship in two conditions: (1) with the water-chamber empty, when the period of the waves was 9·83 seconds, and mean wave slope 1·03 degrees; and (2) with 60 to 70 tons of water in the chamber, when the period of the waves was 9·82 seconds, and the mean wave slope 1·19 degrees. In the first case, the mean absolute inclination of the ship was 5·74 degrees, and in the second it was 3·64 degrees. There

is no material error due to difference in the periods of the waves; and the only error, apart from that which results from the assumption that the rolling is performed steadily, is due to the law of the resistance not depending solely on the first power of the angular speed. Now, if the resistance varied with the square of the angular speed, in the first case the mean angle of absolute inclination corresponding to one degree of mean wave slope would be $5{\cdot}74\sqrt{\frac{1{\cdot}0}{1{\cdot}03}} = 5{\cdot}66$, and in the second case it would be $3{\cdot}54\sqrt{\frac{1{\cdot}0}{1{\cdot}19}} = 3{\cdot}34$, as against 5·56 and 3·06 respectively, given in Table D. Thus the error on this score is not very great, and is small as compared with the possible error involved in the assumption that the rolling is steady.

Notwithstanding the chances of error in the comparison made on page 172 of the rolling of the ship, with and without the water-chamber in use, it appears preferable to the usual method of comparing angles of roll alone, without reference to the waves operating upon the ship.

Note C.

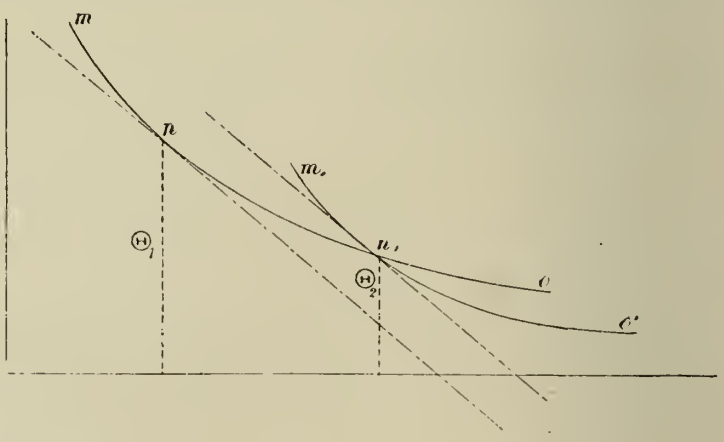
The approximate depth of bilge keels required to produce the same effect as the water-chamber may be estimated as follows:—

The rolling of the ship with the water-chamber empty, at 10.7 a.m., and the rolling when there were (say) from sixty to eighty tons of water in the chamber, may be approximately represented thus:—

I. With water chamber empty. The ship rolling steadily in co-periodic waves of one degree mean slope, to an angle of 5·50 degrees.

II. With sixty to eighty tons of water in the chamber. The ship rolling steadily in co-periodic waves of one degree mean slope, to an angle of 3·60 degrees, or 7·2 degrees from out to out.

The ship was not rolling in co-periodic waves, nor could she be said to be rolling steadily, but these assumptions make the investigation easy, and the result will not be greatly influenced by them.



Now, the rate of extinction of the ship in condition I. in still water, at an angle of roll of 5·50 degrees, must be the same as the rate of extinction of the ship in still water in condition II. at an angle of roll of 3·60 degrees, since in each case the respective angle is maintained by the same waves. Thus if Θ_1 and Θ_2 be the angles of steady rolling in conditions I. and II. respectively, and the curves $m\ n\ n'\ o$ and $m'\ n'\ o'$ the curves of extinction corresponding to these conditions, the tangents to these curves at n and n' which measure the rates of extinction must be parallel to one another; *i.e.*,

$$\frac{d\,\Theta_1}{d\,n} = \frac{d\,\Theta^1}{d\,n}.$$

The effect of the additional resistance in condition II. will be measured by the difference in the rates of extinction in the two conditions at an angle Θ_2, or, what is the same thing, the difference in the rates in condition I. at the angles Θ_1, and Θ_2 respectively.

Let the curve of extinction of the ship without the water-chamber be represented by

$$-\frac{d\Theta}{dn} = a\Theta + b\Theta,$$

then the additional resistance in condition II. will be given by

$$-\frac{d\Theta_1}{dn} + \frac{d\Theta_2}{dn} = a(\Theta_1 - \Theta_2) + b(\Theta_1^2 - \Theta_2^2);$$

and if the additional resistance be supposed to be due to the action of bilge-keels, it will vary approximately as the square of the velocity; hence we may write—

$$-b_2\Theta_2^2 = a(\Theta_1 - \Theta_2) + b(\Theta_1^2 - \Theta_2^2),$$

b_2 being the added co-efficient in the rate curve, expressing the value of the additional resistance experienced in condition II. a and b are known from the still-water experiments with the *Inflexible* (page 170):—

$$a = \cdot 04, \text{ and } b = \cdot 008; \text{ also } \Theta_1 = 5\cdot 50^\circ, \text{ and } \Theta_2 = 3\cdot 60^\circ.$$

Substituting these values in this equation, we obtain—

$$b_2 = \cdot 0165.$$

Let A = area of bilge-keels required to produce the additional resistance.
r = effective radius in feet of pressure on bilge-keels from centre of oscillation of ship.
p = pressure in lbs. per foot of bilge-keels at unit speed of 1 foot per second, on the supposition that the whole of the resistance is produced by normal pressure on the bilge-keels.
T = half the period of the ship in seconds.
W = displacement of the ship in pounds.
M = metacentric height of the ship in feet.

Then equating the work lost by the ship per swing to the work of resistance of the bilge-keels, we obtain

$$\text{W. M. } \Theta_2 d\Theta_2 = \frac{4}{3} p \text{ A } r^3 \frac{\pi^2}{\text{T}^2}\Theta \;;$$

Now—

$$-\frac{d\Theta_2}{\Theta_2^2} = b_2;$$

therefore, substituting and reducing to circular measure, we have—

$$\text{A} = \frac{3\, b_2 \text{M . W . T}^2}{4 p r^3 \pi^2 \frac{\pi}{180}}$$

With regard to the value of p. This will depend very largely upon the positions of the bilge-keels and the shape of the bottom of the ship. In Froude's still-water experiments with the *Greyhound* with and without bilge-keels, the resistance produced by the bilge-keels was the same as would have been produced by a normal pressure upon them of 10·4 lbs. per square foot, when moving at 1 ft. per second. With bilge-keels the curve of extinction was

$$-\frac{d\Theta}{dn} = \cdot 0198\,\Theta + \cdot 0462\,\Theta^2;$$

without bilge-keels the curve of extension was

$$-\frac{d\Theta}{dn} = \cdot 044\,\Theta + \cdot 0032\,\Theta^2;$$

and supposing the difference in the co-efficient b to have been brought about by normal pressure upon the bilge-keels, this pressure per square foot of bilge-keels—obtained by equating the work lost per swing to the work of resistance—would be 10·4 (velocity)^2. Experiments with the *Shah*, with bilge-keels 1 ft. 6 in. deep and with bilge-keels 2 ft. 6 in. deep, gave about the same result; while from the table given by Froude in "Naval Science," vol. iii. page 321, this pressure for the *Volage* and *Inconstant* would be 8·7 (velocity)^2 and 7·2 (velocity)^2 respectively; and in the case of the *Sultan* it would be as low as 1·6 (velocity)^2, approximating to the pressure upon a plane 1 square foot in area, moving through water at the same speed, and held square to the direction of its motion, *i.e.* 1·12 (velocity)^2. The excess of the resistance in the case of bilge-keels over that of a plane moving through the water is doubtless mainly due to the moment of the pressures produced upon the hull of the ship in the vicinity of the bilge-keels; it will be readily seen that the effect of these pressures will depend upon their direction and position with regard to the axis about which the ship is rotating, and therefore upon the shape of the bottom. Now, in the *Greyhound* the bilge-keels appear to have been exceptionally well placed, and probably produced greater resistance from this cause than they would in any other position; but in the *Inflexible*, in order that the vessel may enter existing docks, the bilge-keels must necessarily be kept down, and well under the flat of the bottom, where these forces cannot have effective moment. Making allowance for this, it appears that the value of p for bilge-keels in the *Inflexible* could not be more than 6 lbs.; and this is probably much too high. Substituting this value, and also the values of the other quantities, in the above, viz., $b_2 = \cdot 0165$, $W = 12000 \times 2240$, $r = 35$ (about), $T = \frac{10 \cdot 7}{2}$ and $M = 7 \cdot 8$, we get—

$$A = 1680 \text{ sq. ft.},$$

and therefore the depth of the bilge-keels necessary to reduce the rolling by the same amount as the water-chamber, at the angle of roll considered, would be at least 4·3 feet. This depth, added to the depth of the present bilge-keels, would make about 6·5 feet.

DISCUSSION.

Mr. R. E. Froude: My Lord and gentlemen, I think there can only be one opinion by those present as to the importance of this subject with reference to ships of war, where the steadiness of the gun-platform is of such enormous importance; and where it is often impossible to secure immunity from rolling by the now well-understood method of reducing the initial stability. That is, however, a practical point which I would rather leave to other persons more qualified than myself to enlarge upon. My principal object in rising is to draw attention to some points in reference to the mechanics of the action of the water-chamber in resisting rolling which I think are rather interesting. I am sorry that Mr. Watts has thought it necessary, to save time, to omit a paragraph on page 167, in which he compared the action of water in reducing the rolling to the action of the men running from side to side of the ship increasing the rolling. He points out in the case of the men increasing the rolling of the ship, that they have, in order to operate to the best effect, to be always running uphill, therefore the passage of the men from one side of the ship to the other is to be concentrated as far as possible at the moment when the ship is at her greatest inclination. This proposition can be regarded instructively in two ways. To increase the rolling of the ship and to maintain it require

the expenditure of a certain amount of work, and if men have to produce that work, in order to operate most effectively the men must run in such a way as to do the most work, and that clearly will be done if they are always, as far as possible, running uphill. You may regard them as men working on a treadmill; the further uphill they have to go each run, the greater the height they have to raise their bodies, and the greater amount of work are they able to do. If, again, they run in that manner, their presence alternately on the two sides of the ship will operate most effectively to increase the rolling; because their weight will always be then on the side of the ship going down, and will always be assisting the motion of the ship. The action of the water in checking the rolling is exactly the converse of this. The water is here resisting instead of, as in the case of the men, putting work in the ship. It is absorbing work from the ship, and in order to absorb most work from the ship it should be as far as possible going down-hill, and its weight should be on the side that is ascending. I refer to this principally to call attention to a fallacy which people are sometimes a little liable to get into in first considering a question of this kind. It is clear that the work which the water takes out of the ship must be absorbed in the friction of the water—the action of the water slushing about. Since all the work is to be so absorbed, it might seem at first sight the greater amount of obstruction there is to the motion of the water, the more effective the operation of the water in absorbing the work would be. But the only way in which the water influences the rolling of the ship is by its presence alternately on the two sides of the ship; and the friction of the water must operate to resist the passage of the water from side to side, and so do harm. If there were no friction in the water at all, and if the water traversed from side to side, it would certainly resist the rolling of the ship, just as much as if there was friction in the water, only in that case there would be nothing to limit the degree of swash the water would get from side to side. The effect of friction is simply to limit that action. Another point of importance is this. We know that in the rolling of a ship it is of the utmost importance that the time of the run of the men should be exactly co-periodic with the natural rolling of the ship. Now the motion of the water from side to side of the chamber has a period of its own. If the ship is set rolling artificially, and then made still, the motion of the water from side to side will continue at a certain natural period, and the effect of the water in the chamber to reduce rolling will be most effective when the period of the ship coincides with this natural period of the water in the chamber; so that it is possible the effect of a balance-chamber of certain dimensions would be very much greater for resisting rolling under certain conditions of sea which tend to give a ship a roll of a certain period than if the rolling of the ship were at a slightly different period. Then another point is this. The motion of water from side to side will be least obstructed when it can take the form of a pure wave of translation from side to side of the chamber; but that can only be when the motion is so far moderate that neither end of the chamber is laid bare. If one end of the chamber gets anything like denuded of water the action of the wave of translation will be very much baulked, and the motion from side to side will not be so free. I think it probable that for small oscillations of the ship the balance-chamber will have proportionately a much more quelling effect than for large oscillations. It is possible that in a small sea way, in which the ship would roll a little, that a balance-chamber might be found of very large effect, and in a rough sea, where the ship would roll a great deal, of small effect.

Admiral Sir JOHN HAY: My Lord, I can add nothing to the scientific information on this subject at all, but I can indicate a few questions, which I think scientific gentlemen here might perhaps answer, that would solve some doubts in my mind which perhaps others may also share with me, who have—I will not say as little knowledge—but who are not as well instructed as they

ought to be on the scientific question. The first question is, whether, looking to the limited space in ships, even as large as the *Inflexible*, the object to be obtained, however valuable, is worth the sacrifice of space? I want to ask this question. The object of obtaining the retardation of the rolling by the proposal stated is to obtain a stable gun-platform. No doubt a steady platform would be of the greatest value if the period of the ship and the waves were always regular; because, if the motion of the ship were always in a given direction, the process of shooting flying, knowing where the muzzle of the gun would be at the time the shot reached the muzzle, would be of very great value; but it is not a roll that we have to contend with so much as a lurch. I want to draw a distinction between the periodical times at which a ship rolls, the period of the waves, and the volume and swell of the sea. If that could be dealt with by this process it would give a very great advantage in shooting. What we have to contend with generally is the lurch, and a variety of other motions, which are due to the action of the sea and to other causes. It does not seem to me that the practical value of this retardation of the rolling, which would be very valuable where the motions are always regular, is so great looking to the fact that the disturbing elements are so irregular that they could not be met, in my opinion, by the process of retardation of the rolling as indicated by the lecturer and Mr. Froude. Those are points which I should like to have touched upon by some of those competent to explain them; and if the irregularity of the motion due to the lurch is not to be met by the process of the water-ballast, used as it is here, it seems to me that the sacrifice of space is unnecessary; that the constant and irregular motions to which ships are liable, will not be counteracted to the extent desired by the proposal indicated in this paper.

Mr. H. Morgan: I will just make this remark for the information of Sir John Hay, and any other speaker who may follow. That space can be used, and was used in the *Inflexible*, for carrying the ship's stock of fresh water. Therefore it was not so wasted as Sir John Hay supposes.

Mr. W. John: My Lord, I should like to say a word upon this paper. I have a very high appreciation of the value of it, because it opens out a new field of study of which really we are only now on the threshold. I think that this is the first time that any attempt has been made to improve the sea-going qualities of a ship, or her steadiness at sea, by the use of free water. We all know that free water in a ship does reduce the statical stability. We also know that free water in a ship is a dangerous thing, unless it is well under control; and what occurred to my mind when the paper was being read was this—that it was like dealing with fire, or gunpowder, or any other dangerous thing, and that if it is thoroughly understood scientifically, and followed carefully, experimentally and otherwise, we have an agent which can be of use in naval architecture; but if dealt with at all carelessly it will be a very dangerous thing indeed. I do not anticipate for a moment that in the hands of the Admiralty, or men like my friend Mr. Watts, anything of that kind will occur. So far as the Mercantile Marine goes we have had several illustrations of the effect of free water in a ship in recent cases, one notably a case of the capsizing of a vessel in harbour in still water; and I could mention a good many other cases where, although the capsizing did not actually take place, it very nearly did. On the other hand, we have the effect of free water in a ship at sea. What its effect on the rolling of a ship is I do not know, but I am quite sure of this, that the water did set up motions of its own, and did not accommodate itself to the rolling of the ship in such a docile manner as Mr. Watts has assumed. It was in large double water-ballast tanks. I have one case in my mind very clearly now where there was a leak in the tank; the vessel was crossing the Atlantic with water-ballast in the tank and the water got down; the captain, instead of pumping the tank up, commenced to pump it out, and it got down a certain distance, and the water set up motions of its own, and it became very wilful when it first got to the top of the tank. It was

supposed that the tank was weak, and it was strengthened (I think it was in New York), and additionally pillared, but the original defect was not remedied; the original mischief was the leak in the tank. The water got down a certain height, and the ship got into heavy water, and this water got into violent motion again and started the top of the tank the second time, and there was a middle-line bulkhead in the tank, and it smashed that completely. That was on her next voyage. I just give this as an illustration of what I stated, that I can quite believe water, properly under control, would have the effect indicated in this paper; but if it is not properly under control, and if you do not study it very carefully, it may be a source of mischief. I will only again express my appreciation of the subject, because I quite foresee a field of study in it; but I think we are only now on the threshold of a very important line of investigation.

Sir Edward Reed: My Lord, I think I gathered from Mr. Watts that this use of water for checking the rolling of ships was deliberately introduced into the Royal Navy for that purpose, because the beginning of his paper almost intimates that; but I am at a very great loss indeed to reconcile that with a mass of facts which occur in connection with the *Inflexible* herself. What I understood, or at any rate what we were told, on, I think, sufficient and multiplied authority, was, that the *Inflexible*, having been designed, and possessing certain characteristics and qualities, in the course of experiments made in connection with water, it was discovered that the motion of free water in the ship had great effect upon the rolling motion; but that it was hinted that this was originally part of the idea of the design of the *Inflexible*, is novel to me. If Mr. Watts says it is so, of course it is impossible for me to contest that for a single moment. The only remarkable circumstance is, that in all the many official papers published about the *Inflexible* when she was designed, constructed, and under discussion, we were never made acquainted with the fact, and only learn now, in 1883, that this was introduced as a feature of her design originally. It is very important that the Institution should know whether they are dealing with an application of an incidental discovery, or whether they are dealing with a deliberate invention, adopted in the Royal Navy for specific purposes. I thought it worth while just to mention that point, but I do not think it worth while to insist upon it. What I do think of importance is, that we should get from the Admiralty the fullest information from time to time which can be furnished to us on this subject, because I regard the question as a very serious one, looking to what undoubtedly happens at sea, and has often happened in my experience, which is that, agreeable as it would be to scientific men to have uniform waves pass in uniform succession, and always to be depended upon to come at the right time and in the right shape, instead of that, what you get at sea is unexpected mountains and deep holes of an exceptional kind by comparison with the general waves, and a general malformation, disturbance, and puzzled condition of water which does not seem to follow any law at all, except the law that whenever one disturbance encounters another it takes its full effect as if it had not encountered it, and piles the one on the top of the other, until you get conditions which set up in the ship the greatest possible embarrassment, resulting in extreme lurches to one side, and in the ship not unfrequently lying there a good time in the extreme position. Now it does appear to me, and it did before Mr. John spoke and reminded us of certain facts, that we may induce people to do very dangerous things if we do not mind, in dealing with this question. Of course Mr. Watts deserves our thanks for giving us all this information, and the Admiralty also deserve our thanks for having allowed this information to be given to us; but I do not know that I have ever before felt, with reference to any paper communicated to this Society, that it so strongly imposes, both upon the Admiralty and the scientific advisers of the Admiralty, the duty of following up the subject with further information. I listened very carefully to Mr. Watts, but, unfortunately, he did not read the

paper, except in parts, and probably from that cause I did not get a full and clear impression of its purport: but it does appear to me that the paper does not touch, sufficiently at any rate, the dangerous elements involved in the use of free water in the ship; and, of course, it is a very serious thing to have a paper read upon a great system adopted by the British Admiralty, and carried out in the most important ship of Her Majesty's Navy, and described by an officer of the Admiralty at this Institution, if we are to have its favourable and meritorious aspects presented to us, and not also to have its dangers pointed out. We are in the enjoyment, I believe, as an Institution, of very great influence. There are, as we know, distinguished persons, both English and foreign, who attend our meetings, and I can conceive very great harm being done if a false impression were conveyed as to the innocence of the use of free water in large bodies in Her Majesty's ships. When I saw the title of this paper I was very anxious to have the question developed and discussed, or rather to get information upon it. I wanted to know what happened when you allowed a large volume of free water to dash from one side to the other of a ship 70 ft. broad? The paper does not throw much light on the question, but it throws some, because it tells us that it began to dash about with such violence, and with such a total absence of all that amount of propriety which seems to have been expected, that the tank had to be closed before they got very much in it, and after that it became very difficult to make any trustworthy observations as to the quantity admitted; so that evidently it is such a difficult element to control that it is not easy to get accurate information about it. I feel most strongly that this Institution has a claim, in this case, to have the fullest information from time to time which can be furnished respecting it; and I hope that this paper of Mr. Watts, giving us, as it does, no doubt, all the information in his possession, will stimulate a great many persons to think the subject over, and to treat this as what it should be considered, the mere starting-point of a new inquiry. Mr. John, very properly, I think, compared it to fire and gunpowder, but this is not an Institution in which we are prepared to taboo either fire or gunpowder; it is one in which we recognise their value; and I am sure that the Admiralty officers, and scientific members of this Institution generally, will feel that the fact of its being a difficult element to control, and sometimes a dangerous one, is no reason why the subject should not be developed, if it is proved to have real value in steadying ships at sea.

Mr. F. K. Barnes: The discussion on this paper seems to have gone away from its real object to the danger of free water in a ship. We always have known that there was very great danger in free water, and have endeavoured, in the use of water-ballast, or other water in a ship, to control it, and to keep it confined to its proper place; not to let it be free, in fact, nor to let it drift about. In the case of the *Inflexible*, it is contained in a large tank. If you look at Fig. I., Plate XII., you will see the size of the tank. When you consider the size of the tank, and bear in mind what the displacement of the *Inflexible* is, I do not think anybody would imagine that any very great harm would be done accidentally by the free water. The water is placed in a very strong tank, and I cannot imagine that any harm is likely to attend its use. It is not as though it was placed in a tank of very large area. I forget for the moment what the length of the tank is.

Mr. P. Watts: Fourteen feet.

Mr. Barnes: Of course, a large area is the dangerous feature in free water. It is not the quantity, but it is the area over which it is allowed to extend. If that tank extended right down to the ship's bottom it would not increase the damaging effect of free water. Supposing it were so carried down, there would be no more danger although it contained three times the quantity represented in the drawing—120 tons.

Sir Edward Reed: I do not know that.

Mr. BARNES: What has been done here is to show that a small quantity of water can be used for a useful purpose. If it does not do its work, it cannot do any harm. I mean to say that its action must be of the nature of a frictional force, and when it does its work, it does its work in the right direction.

Mr. B. MARTELL: My Lord, I should like to make a few observations with reference to the subject of the danger of free water. I think we are very much indebted to the Admiralty for their having brought before us these experiments. We are accustomed to look carefully now to information supplied by the Admiralty, and to the effect that it may have on the Mercantile Marine. As Sir Edward Reed says, it would be unfortunate if we were to sanction by any remarks we make, an element of danger such as this, supposing it to be so. I can fully confirm all Mr. John has said on this subject. Free water in a ship is an element of very great danger, and although we have in our merchant ships' tanks extending 20 ft. instead of 14 ft., or more than that——

Mr. SAMUDA: This is 14 ft. broad.

Mr. JOHN: No; 14 ft. long.

Mr. MARTELL: It runs across the ship; but it is 14 ft. long.

Mr. SAMUDA: Yes; forward and aft.

Mr. MARTELL: Yes; fore and aft. Some tanks extend up to the hold beams, 14 ft. high and 20 ft. long, and some contain as much as 400 tons. This would contain about 130 tons, so that in a very strong ship such as this, there is not the same danger as there would be in a merchant ship with a large quantity like 400 tons. I cannot conceive, as Mr. Barnes says, in this particular instance, that a great deal of danger would occur; but in merchant ships, where these tanks are fitted, we have to be exceedingly careful to see that they are strengthened up in a proper manner. In the early tanks that were fitted, where the bulkheads had merely angle irons to stiffen them, and there were angle-iron beams at the top of the tank, it was found from experience that those tanks were perfectly incapable of withstanding the concussions of water where there was a little leakage, and care was not taken to fill the tank up. Sometimes there is a danger in that, occurring where the ship is trimmed by the stern, and the tank is run up; unless you fill it from the top, there is a difficulty in getting it brim full, and where these large masses of water are moved about in that way it is an element of exceeding danger. There is consequently great necessity, where tanks are fitted for the purpose of carrying water ballast in that way, to exercise the greatest care to see that they are completely filled and kept filled. We have in all these cases required that there should be a partition bulkhead at the middle line, to prevent the danger that would arise from the wash of the water from one side of the tank to the other. I say this in order that it may not go forth that free water can be adopted in ordinary mercantile ships without great danger arising from it, and without the necessity of the greatest care possible in the use of it.

Mr. J. D'A. SAMUDA: My Lord, I should like to speak upon this subject, because, as Mr. Martell and Mr. John have observed, what we have been doing up to the present time has been entirely to do away with what this paper recommends us to adopt for the safety of ships. We have considered, and have found the greatest possible mischief arise from having water free in tanks when used as ballast; and I have very great difficulty in speaking at all, except to say this, that I am fortified in making these observations when such men as Mr. John and Sir Edward Reed fail to see, as I fail to see, from the description given to us, the good effect which it is desired to impress

upon us would result from changing the old style and adopting the free water instead of the confined water. To such an extent do we find confined water necessary in practice, that I have myself seen vessels fitted with water-tanks, in the greatest possible danger from not being absolutely filled when at sea; and the action of the sea has tended to make them still worse, for the simple reason that the movement of the ship lurching on the one side is followed by the water, and produces the same result as the shifting of cargo to the side the ship is lurching to. What is the difference? No doubt there is some difference, or Mr. Watts would not have taken the trouble he has to give us all these particulars, but I fail myself to see that he has carried conviction to our minds of one especial thing; that is to say, that when the ship was rolling by the action of the sea in one direction, say to starboard, that the action of the water could in any shape or way have reduced that rolling. It appears to me that the action of the water coming down a hill must have accelerated that result. There may have been a period when it was changed, and the water would have followed the ship in the opposite direction, but in the first instance it must have aggravated the mischief. At any rate, I agree with Sir Edward Reed very much indeed. It probably may be my obtuseness in not being able to follow the paper, because it is a difficult paper to follow at once, but it does appear to me that when a matter is propounded, as this is, totally at variance with the generally received experience of the mode of using water in ships, we should hope and expect to get some much clearer definition of how this is acted on than I have been able to get from hearing this paper read.

Mr. Barnes: My Lord, perhaps I might be permitted to say a word by way of explanation. Mr. Martell, Mr. John, Sir Edward Reed, and Mr. Samuda take one view, we take another. The difference comes from this circumstance: they have generally to deal with ships of small stability. We have in this case to deal with a ship where there is a great excess of stability. There is no difference at all between us as to the effect of the water, it is only the difference in the ships with which we have to deal.

Mr. H. Morgan: Might I explain just another point? No doubt gentlemen have had in their minds masted sailing ships when they were speaking; but evidently this plan would not do very well for a sailing ship, because, when the ship was listing under a wind the water would be all on one side, and the angle of heel would be increased, while owing to the reduction of free surface the checking action of the water would be diminished. The *Inflexible* is purely a steam-ship, and therefore the objection I have referred to would not arise.

Sir Edward Reed: I did not think of sailing ships.

Mr. W. W. Rundell: I rise with some diffidence, for I consider it to be a great honour to be allowed to join in the present discussion. In the first place, I would allude to a matter that has struck me very frequently since Mr. Froude read a paper to this Institution, many years since, on the rolling experiments on board the *Devastation*, when the *Devastation* and the *Sultan* went to the West of Ireland for the purpose of testing an instrument for measuring the oscillations of the former among waves. It will be remembered that the vessel had to remain there, I think, for three weeks—or it may be more—perhaps you can correct me, Mr. Froude?

Mr. R. E. Froude: I don't know. I was not on her at that time.

Mr. Rundell: I think I am correct in saying that she had to stay at sea three weeks before a suitable opportunity occurred of testing that instrument, and it arose from this cause: a cause that underlaid the remarks made, I think, by Admiral Sir John Hay at the beginning of our discussion. To test that instrument accurately it required that the waves should be ruled, as

it were, to equal intervals. Now this occurs very rarely. We get simple waves but very occasionally. Ordinarily waves are compound ones, and there is no harmony, as it were, between their components. If you turn to the diagram, Fig. VII., it will be seen that the nodes of the curves correspond very nearly at certain intervals, that is, they are harmonic to a certain extent.

Mr. WATTS: These curves were produced by natural waves, and the sea was confused.

Mr. RUNDELL: I quite understand that, but the oscillations we see recorded upon the diagram are nearly symmetrical. The wave period, the time of the ship's roll, and the time of the movement of the water in the chamber, are very nearly the same. Now, this leads up to the remark that this investigation, though exceedingly valuable theoretically, cannot really have a very practical application unless it can be shown that it applies not only to simple waves or to harmonic waves, but also to the very irregular waves which are commonly met with at sea. I can conceive it quite possible that occasionally, once or twice perhaps, in one hundred years we may get a series of compound waves where you would get this harmonic combination, but unless you get this harmonic combination it seems to me doubtful whether the means supplied would show the requisite results. I should like to ask Mr. Watts whether, if the apparatus were used among irregular waves, it would have the effect of checking the oscillation, or whether its use is confined entirely to those simple waves that Mr. Froude waited so anxiously for on board the *Devastation*. As regards the possible injury from free water on board ship, no one will question that, but it has occurred to me that perhaps, instead of leaving a portion of the chamber empty, if there were a very large elastic bag in it so as to partially fill up the air space, we should get two advantages; first, a motion of the water that could be regulated so as to correspond to the observed period of roll of the vessel; and, secondly, we should get a great deal of friction, which might be of use in compressing the elastic bag on alternate sides in the chamber. At all events, we should get rid of the chamber being so loosely filled as to leave the air and water both free. I just throw that out as a suggestion, my Lord, and I am very glad to have had the opportunity of making these remarks.

Mr. WATTS: My Lord, with regard to Mr. Froude's remarks, I may say that during the deeper rolling on this occasion the ends of the chamber were successively laid bare, or nearly so. I quite agree with his remarks. As long as the ends are not laid bare the chamber will probably be most effective when the period of the water is the same as that of the ship; when they are laid bare this may not be quite the case, so that the period question is probably more important for small angles than for large. With regard to what Sir John Hay said as to the motion of a ship rolling at sea, the transverse motion is fairly uniform, however irregular the waves may be; the time of each roll is about the same.

Sir EDWARD REED: Oh!

Mr. WATTS: On this particular occasion the sea was very irregular indeed, yet we got these curves (Fig. VII.), in which the time of each roll does not differ very greatly, and where there are no sudden changes of velocity. The curve A A A represents the inclination of the ship to the wave normal, and the curve B B B the inclination of the ship to the true vertical; so that the points where these curves cross correspond alternately to wave crests and wave hollows; and it can be seen that the waves, over the time here taken, were very irregular both in period and height. There are no shocks or lurches indicated, if by these are meant abrupt changes in the velocity. The shocks communicated by the sea striking the ship, simply produce tremor or vibration in the ship, and influence the movement of the ship as a whole, but they do not produce sudden change of velocity and make local disturbance in the curve.

Sir EDWARD REED: If I might interpose, I think that needs a little fuller explanation. You said that whatever the state or the variations of the sea might be, the rolling of the ship was the same.

Mr. WATTS: Not the same in extent. I mean, the rolling would be performed in about equal times, or at any rate the times could not be very greatly changed. The ship could not, I think, be detained for a lengthened interval at the end of a roll by wave action. Fig. VII. is not a part especially picked out because the rolling happens to be performed in nearly equal times; the whole of the records taken during the day are of the same general character as this, and so are the whole of the records that were taken during the trial cruises of the *Devastation*, although the seas were often not more regular, and often far less nearly co-periodic than those which produced this rolling. The curves do not show lurches, they are nearly continuous harmonic curves.

Sir EDWARD REED: That must be from a defect in the instrument. There are lurches.

Mr. WATTS: There are no abrupt changes.

Sir EDWARD REED: I beg your pardon.

Mr. WATTS: With regard to the remarks made by Mr. John and Mr. Martell, as Mr. Barnes has observed, this paper deals only with ships which have very large stability, and where the object is to reduce the stability as far as this is possible consistently with other requirements, or, at any rate, to bring into operation forces which will have the same effect upon the rolling of the ship as reduction of stability would have. I do not recommend water-chambers for ships which have only sufficient stability to make them safe and comfortable, and I quite agree that free water in such ships may be a source of danger. The cases quoted by Mr. John and Mr. Martell, and also those referred to by Mr. Samuda, are not similar to the case of the *Inflexible*, and their remarks upon these vessels do not, I think, apply to the class of vessels referred to in my paper. With regard to the remarks made by Sir Edward Reed, I may say that the water-chambers were designed originally as part of the design of the *Inflexible*.

Sir EDWARD REED: For that purpose?

Mr. WATTS: For that purpose, and only for that purpose, so far as their design was concerned.

Sir EDWARD REED: Then it should not have been concealed.

Mr. WATTS: The chamber retained can be used for other purposes. Besides being capable of being charged with fresh water when required for steadying the ship, which can be used as a reserve, it can be used when not so required for carrying stores; but the chambers were intended to be used as water-chambers, and were set apart for that purpose in the course of the design of the ship. Of course the chamber must be made strong enough to hold the water. I think there can be no doubt that, no matter how the water may be dashed about in the chamber of the *Inflexible*, there will not be any fear of its getting out; but even if it did, it would do no great harm, and would not endanger the ship in the least, although, doubtless, it would be very inconvenient. It might wet a few people through and spoil stores, but this is about the limit of the harm it could do. This was what I referred to when I said that the hatch could not be kept open. If it had been kept open we should have been wet through, or those who were in or near the hatchway would. The water passed from one end to the other, and striking against the partial bulkheads, dashed back heavy spray, so that it was no use looking down, as one was blinded with the spray. I am sorry I did not make myself more clear to Mr. Samuda, in reading my paper. I shortened up some parts to

get it into the prescribed time, and in doing so made them, I fear, less clear. The effect of the water in the water-chamber of the *Inflexible* is, as I have shown, to reduce the rolling of the ship to an extent which I venture to think makes it a valuable feature in this ship; and it appears to me that a water-chamber must, if properly designed, always tend to reduce rolling; free water may, however, make a ship dangerous, and it would probably do so if carried in a ship possessed of but little stability. I do not say that the effect of the chamber may not occasionally be to increase individual rolls; but this will be exceptional, and in the aggregate the effect will be to diminish the rolling. Now, looking at Fig. IV., Section No. 1, here the ship has reached the extreme heel to port, and the water is running down on the incline. We will say the centre of gravity of the water has attained the position of "g," and in that case the centre of buoyancy and the centre of gravity have the positions due respectively to the immersed form of the ship, and the disposition of the weights where the water is a fixed weight amidships, and the righting force is not affected by the water-chamber. Passing to Section No. 2, the water has had more time, and the centre of gravity has moved further down the incline and has got to the position of "g," in that figure, and consequently the centre of gravity of the ship and the water combined has moved out from the middle line on the port side: the centre of buoyancy, of course, takes the position corresponding to the immersed form. The result of this is that the righting force is less while the ship is returning towards the upright position, and therefore less angular velocity is accumulated when she reaches the upright position. In Section No. 3, she has reached the upright position, but still there is a righting force tending to reduce the rotation, due to the fact that the centre of gravity of the ship and water has not returned to the middle line. The righting force impelling her towards the upright position is always less than it would be without the water-chamber. And continuing the motion on to Section No. 4, the ship heeling to starboard, the water has not had time to return to the middle line, and consequently the centre of gravity is on the port side still, and the righting force is greater than it would be if the water in the water-chamber were simply a weight fixed amidships. Thus the righting force, which may be regarded as regulating the rolling, is continuously modified so as to diminish the rolling. The water must move in this way, it cannot help it, if the chamber be properly designed.

Sir Edward Reed: While the ship is descending and the water is descending.

Mr. Watts: The ship has to descend before the water has any tendency to descend. The ship must precede the water unless the angle is so small that the question of the period of the water becomes of great importance, and then if the periods are the same the water will lag behind the ship.

Mr. Martell: To avoid which motion it would be necessary to have a shallow chamber and a broad ship.

Mr. Watts: That is exactly what we have—a shallow chamber and a very broad ship. As I have stated in my paper, we do not profess to possess complete knowledge of this subject; we are only at present investigating it. It was hoped that the experiments in the Mediterranean would have settled most questions bearing upon the subject; and there is no doubt that more complete data would have been obtained if the experiments could have been completed. The experiments we now have in hand, and which have grown out of those I have described, will probably settle the various points. I trust, however, that the suggestion made by Mr. John will be acted upon, and that the members of the Institution will give this matter their consideration, and that we may have the advantage of their investigations. I may say that this was one of the objects had in view in bringing the subject to your notice.

Sir Edward Reed: I should like to say one word in explanation, in order to prevent public

misapprehension. I wished to say in my remarks that I did not intend to imply that there was any danger to the *Inflexible* from the use of water in her tank. I had never such a thought, but it was merely that wrong inferences might be drawn by other people in other cases, and that might prove very dangerous.

Mr. WATTS: I have only to thank you, gentlemen, for the kind way in which you have received my paper.

The PRESIDENT: I am sure, gentlemen, you will permit me to convey our thanks to Mr. Watts for his very able paper upon what must be to all of you one of the most interesting, and at the same time one of the most novel subjects that can be introduced to your notice. I should not have ventured to ask you to allow me to say a single word upon so difficult and so scientific a subject, but good luck happens to everybody now and then, and I had a conversation on this very subject with one of the most experienced of the Cunard captains, who has crossed the Atlantic probably as often as any man living. I had a long conversation with him upon this subject at the time the experiments were going on, and I daresay you will let me, in two or three words, tell you what he told me. He said: "The force of wave motion and its effect upon ships will never be ascertained, I believe, thoroughly satisfactorily. I have stood on the deck of my vessel many times with a measure in my hand. I have taken the length of the waves, and could calculate the roll, and I have taken down accurately day by day the result of my observations, but I have never been able to satisfy myself upon any theory that I could recommend, because there are so many disturbing influences, so many sudden motions which are not foreseen, but which affect any calculation." This man is an extremely clever man. You can see from what little I have said that he also is a scientific man. "But I have never been able to satisfy myself upon any theory that I could satisfactorily recommend to the public attention." I think the subject is so important that I may be pardoned for just mentioning that circumstance. I was glad to hear my friend, Mr. John. He did not name the vessel that he alluded to, but we probably all know what it was. He referred to that terrible calamity that occurred recently to one of the finest ships in the merchant service, which left the Clyde hardly a year ago; I mean the *Austral*, belonging to the Orient Company, and which foundered in coaling in Sydney harbour. I have watched carefully, and no doubt you have all watched carefully, the very scanty information which we have yet received, but the only explanation made as to the cause of that extraordinary and unprecedented calamity—there never was such an instance since the *Royal George*, I believe—is this: that she carried 800 tons of water ballast. How much of it was left behind, not pumped out, nobody knew, but there can be no question that a considerable portion of it was left behind, or else there is no possibility of accounting for the accident, and the only suggestion which is made yet is that a certain proportion of the water ballast was left in, and that this "free water" assisted materially the fatal lurch which, owing to great carelessness and neglect at the time, brought the coal-ports down to the water level and sent that glorious ship to the bottom. I think that is so important an accident that Mr. John will forgive my pressing it a little more home, but it is a recent case, within all our recollections, and until the inquiry is held we shall never know the cause of it. However, that is the suggestion made, and the only one that probably could account for such an accident happening under such circumstances. I thank you, gentlemen, for allowing me to make these few observations, but I think they do bear on the subject; and I now beg to offer our thanks to Mr. Watts for his most valuable paper upon this most interesting subject.

TONNAGE MEASUREMENT, MOULDED DEPTH, AND THE OFFICIAL REGISTER IN RELATION TO THE FREEBOARD OF IRON VESSELS.

By W. W. Rundell, Esq., Secretary to the Underwriters' Registry of Iron Shipping; Associate Member of Council.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.] *

The much-discussed topic of tonnage measurement will only be referred to in this paper so far as it relates to the question of freeboard. Under-deck tonnage is an element which has been introduced more or less directly into all schemes for indicating on a ship's side percentages of spare buoyancy. A ship's registered tonnage is, however, of no value for this purpose any further than it approximates to or bears some fixed proportion to her total displacement. Until recently the proportion between the two has not differed very widely. Occasionally an owner has had deeper floors or thicker ceiling than was customary, and has thus been able to carry more weight than his neighbours on the same registered measurement. Where one vessel has barely carried what is termed "registered tonnage and a half," another has been reported to carry "tonnage and three-quarters." These were sailing vessels. In steamers this cause of difference in carrying power is concealed by the legal deductions, which depend chiefly on the manipulation of engine and boiler spaces, but these deductions do not alter a steamer's under-deck tonnage, and consequently do not affect our present subject.

There has also existed a small advantage in the weight-carrying power of shallow iron vessels, both sailing and steam vessels, over those of deeper proportions, owing to the relatively larger space which is occupied by the floors of the former. These differences between ship and ship which have now been alluded to were never so great as to call for much public notice, although they clearly affect the strict accuracy of our tonnage statistics. Recently, however, by the introduction of double bottoms on the longitudinal construction in steamers the case has been much altered. In these vessels the difference

* This paper, in its original form, was presented to and partially read at the meeting of the Institution of 1882, but was not inserted in the printed Transactions for that year.

between the internal and external measurements is so great that neither under-deck tonnage nor registered depth of hold is any longer a satisfactory guide to their carrying power and freeboard. A continuous double bottom of twice the depth of ordinary floors is not at all unusual. A good many vessels of this type have been built, and are now building. Probably about one out of every eight steamers built during the past year was of this kind. Something more, therefore, than is now recorded in the official register of such vessels is requisite for estimating their freeboard.

How can this deficiency in the official measurements be amended? What other recognised measures can be substituted for them?

Among the first recommendations made by the late Tonnage Commission were proposals to give greater freedom to the official measurers; to make new rules for measurement suited to the present state of shipbuilding; to make regulations for fixing a normal or standard depth for floor plates.

Their motives for making these recommendations were evidently a desire to retain the Moorsom system of internal measurement, to equalise the incidence of tonnage dues, and to avoid any break or irregularity in the general tonnage statistics of this country. One may reasonably doubt the propriety of attaining either of these desirable ends by fixing a standard depth for floor plates. Why not also regulate the moulded depth of frames, the thickness of ceiling and side battens, and the thickness of the third and fourth decks in very large vessels? Each of these affect in some way the question of under-deck tonnage. If a standard of depth for floors were fixed by Act of Parliament it would no doubt be a maximum depth But would not this stimulate the ingenuity of the shipowner and naval architect to the construction of very shallow floors to suit those who required space rather than displacement, and to gain this space without a corresponding payment of tonnage dues? I say nothing in condemnation of ingenuity of this kind, but I should wish a great deal to be said to discourage legislation upon such technical details. Such a course seems to me to be wrong in principle, and of very doubtful success.

The remedy I wish to submit for your consideration avoids evils of this kind, and will leave the owner, the shipbuilder, and I may add the registry societies also, quite free from any bias affecting tonnage measurement. By it the basis of our statistics for under-deck tonnage will be made more uniform with the past and more accurate for the future than does the present practice I propose to attain the desired end by a simple method which rigidly retains the principle of making one hundred cubic feet equal to one register ton.

If we take a number of vessels of the ordinary construction and group them together according to their tonnage, it will be found that the following ratios exist between their internal measurement and their total displacement :—

Tonnage of Vessel Under-Deck.	Ratio of Inside and Outside Measurement.	Percentage upon Under-Deck Tonnage.
300	1,000 : 1,160	16·0
400	1,000 : 1,154	15·4
500	1,000 : 1,148	14·8
600	1,000 : 1,142	14·2
700	1,000 : 1,136	13·6
800	1,000 : 1,130	13·0
900	1,000 : 1,125	12·5
1,000	1,000 : 1,120	12·0
1,100	1,000 : 1,116	11·6
1,200	1,000 : 1,112	11·2
1,300	1,000 : 1,109	10·9
1,400	1,000 : 1,106	10·6
1,500	1,000 : 1,103	10·3
1,600	1,000 : 1,101	10·1
1,700 and above	1,000 : 1,100	10·0

Adopting these figures, or amended ones if better can be found, we can get from total displacement a much more uniform method of obtaining under-deck tonnage than the present system of measurement permits—one which will agree with the past in its results, and one not liable to change in the future by changes in the mode of construction of iron vessels.

Under-deck registered tonnage will be obtained by multiplying total displacement in cubic feet by the proper fraction as indicated in the above table, and then dividing the quotient by the present divisor of 100.

The measurement of tonnage above deck also belongs to our subject. The Tonnage Commission do not propose any alteration in the present method of measuring these spaces. But objections have been made, at our meetings and elsewhere, to placing the same value upon spaces enclosed by erections above deck which is placed upon equal spaces under deck. To do so is said to cripple the shipowner unnecessarily, and to prevent his making improved arrangements for the safety of his crew. Whether these allegations be assented to or not, it is clear that the relative value of above-deck and under-deck space must of necessity be assessed for freeboard purposes. Perhaps it is not of much consequence whether the assessment battle has to be fought out on behalf of freeboard, or for the more equitable payment of tonnage dues.

Our next topic is moulded depth. A vessel's registered depth of hold has never been more than an approximate index to her real depth, but its entry upon the ship's official register, and the ease with which it can be ascertained, have made it a very convenient factor in estimating freeboard. In the new type of steamer, as depth of hold bears no regular relation to the actual depth of the vessel, moulded depth becomes of immediate importance. Under the proposed mode of obtaining under-deck tonnage, a record of

depth of hold would no longer be needed, and moulded depth would naturally take its place on the official register.

Here I would ask you to call to mind a certain relation between moulded depth and total displacement to which I directed your attention in 1874. The chief point is that in vessels of medium form 30 per cent. of total displacement corresponds exactly to 25 per cent. of moulded depth. We have thus a simple mode of cutting off or indicating 30 per cent. of spare buoyancy in all vessels which approach the average form, and as there is usually very little difference in a vessel's displacement per foot above this point, it becomes very easy to indicate any smaller percentage. In the same paper I also showed how percentages of buoyancy could be measured on a ship's side with a considerable degree of accuracy up to 40 per cent.

Fig. I. (Plate XIV.), which is mainly a repeat of one which appears in your Transactions for 1874, will illustrate my remark, and at the same time elucidate some additional observations. The line of figures at the top shows percentages of total displacement, and other figures placed just below percentages of reserve buoyancy. At the side the figures show percentages of moulded depth, and freeboard in percentages of moulded depth. Thus 90 and 80 per cent. of displacement are reciprocally equivalent to 10 and 20 per cent. of buoyancy, and 90 and 80 per cent. of depth are reciprocally equivalent to 10 and 20 per cent. of freeboard. What I have termed moulded depth in the diagram has a small addition made to it, so as to include displacement equivalent to that which round of deck and sheer of the vessel adds to her moulded depth displacement. In the same way, the displacement due to round of deck and sheer is also included in the 100 per cent. of total displacement.

The straight diagonal line will of course represent the displacement of a cube, or any rectangular figure like an ordinary box. The dotted curves furthest to the left are those for the displacement of a triangular prism, angle downwards, and for a hemisphere or for a semi-ellipsoid. The thick curve B, between this last curve and the diagonal line, represents the mean of the displacement curves of twenty-five vessels of very various types, and by several different builders. By carrying the eye down the percentage of buoyancy line 30 it will be seen that it intersects the percentage of freeboard line 25 exactly upon this central curve. The details of each of the twenty-five curves will be found tabulated in the volume of our Transactions for 1874. The displacement curve A on the diagram represents vessel No. 8 in that table. The line C is the displacement curve for the vessel No. 20. The curves for all the other vessels, as well as those for several vessels which I have obtained since, all fall between these two extremes—between the curves A and C.

It will be obvious that if we knew the special type of vessel represented by each of these three lines, A, B, C, and could easily refer any given vessel to these or to intermediate types, we could approximate very closely to the freeboard representing any desired percentage of buoyancy, and this without seeing the actual displacement curve belonging

to the vessel, *i.e.*, if we knew her moulded depth and the usual allowance for round of deck and sheer.

At first sight of this diagram it might seem that the ordinary co-efficients of fineness would represent the types of form already spoken of, and that supposing the curve A represented co-efficient ·60, curve B represented co-efficient ·70, and curve C represented co-efficient ·80, we could easily interpolate curves for any other co-efficients, and we should then be able to mark off, both readily and accurately, a freeboard which would correspond to any percentage of buoyancy if a vessel's co-efficient of fineness were known.

A scale of freeboard, apparently prepared on this assumption, and circulated among persons interested in shipping with a request for their remarks, seemed to me at first sight to give in this way fairly satisfactory results, but a little further investigation showed me that this hypothesis was not a tenable one. The more it was scrutinised the more anomalous appeared the results. The cause of this will be seen by another glance at the diagram. The figures which are placed upon the displacement curves show the co-efficients of the vessels which respectively belong to them. The displacement curve A which approached nearest to that for a hemisphere, of which the co-efficient is ·52, turned out to be ·70, while that for co-efficient ·82, vessel No. 15 in the table, which might be expected to fall nearest to the diagonal line, will be found near the centre of the diagram. The curve nearest to the diagonal line is that for vessel No. 3 in the table, and has a co-efficient of ·65. The thick central curve equally well represents, for different vessels, co-efficients varying from ·64 up to ·80. Lastly, I took two vessels, each 300 × 33 × 24 moulded depth, one of about 200,000 cubic feet of total displacement, the other of about 150,000 cubic feet, so as to represent co-efficients ·80 and ·60 respectively. I adopted freeboards of 5·2 feet and 4·54 feet (with the addition of 5 per cent. of moulded depth as an allowance for round of deck and for sheer of vessel) and the displacement curves fell, one nearly upon the central curve, and the other just outside the curve which had previously been the nearest to the diagonal. This little experience led me to a further examination of the connection between moulded depth and surplus buoyancy.

It will, I trust, be remembered that at our annual meeting in 1875 I pointed out that the usefulness of tables of freeboard like those which were presented in 1873 to the Royal Commission on unseaworthy ships by the Chief Surveyor of Lloyd's Register, or like the slightly modified edition of these Tables which appeared in our Transactions for 1874 (pages 102 to 129), or any similarly constructed tables, depended entirely on the few figures which headed them. It is somewhat discouraging that, after the lapse of eight years, I should have to repeat the identical words I then used. Present circumstances, however, compel me to again say that the potency of these or any similar tables depends altogether on two lines of figures, the one expressing percentage of reserve buoyancy, the other the depth of vessel in feet and inches. The columns for co-efficients of fineness and the hundreds of entries for freeboard are, in my opinion, entirely superfluous.

I propose now to attempt so to demonstrate my assertions as to convince every naval architect, and I hope every amateur who may be present, that they are correctly founded.

I shall to this end seek to establish two points. The first relates to the co-efficients of fineness. We have already seen, by Diagram No. I., that these co-efficients have no fixed relation to ordinary curves of displacement when expressed in percentages of total displacement and of moulded depth. I now wish to show that it is possible for the same curve of displacement when expressed in percentages to truly represent vessels of every degree of fulness or fineness from co-eff. ·60 to co-eff. ·80. The curves, as already intimated, show the mode in which the displacement for different forms increases with each unit of immersion. Thus the square box (see diagonal line) for each increment of immersion, or depth, increases throughout by equal increments of displacement. The triangular prism, with one angle downwards (Curve D), increases the displacement much more rapidly; at the second unit three times as much as at the first; at the third unit five times as much; at the fourth unit seven times, and so on to the end. And this is true whether the downward angle of the prism be acute or obtuse, all we have to consider being the rate of increase. If the triangular prism be immersed with the base of the triangle downwards the curve E is obtained, and if the end of the prism be placed downwards its displacement will, as in the case of the rectangular figure, be represented by the diagonal line. Each curve of displacement indicates the rate of increase of the displacement of the vessel as unit by unit she is submerged. Now, if we take a vessel of a given moulded depth, say of co-efficient ·70, and regularly increase or decrease the areas of every water line from the deck to the keel in the same proportion, it is evident we may obtain vessels for co-efficients ·60 and ·80 which will have identically the same displacement curve as the one with co-eff. ·70, providing always that the same proportion of total displacement is retained for sheer and round of beam. It will also equally well represent vessels corresponding to all the intermediate co-efficients, for by hypothesis their displacements all increase in the same proportion throughout. Consequently, in all these vessels the same percentage of buoyancy will be cut off at the same height, and any tables which give a different freeboard to each of them and for the same percentage of displacement, clearly attempt that which is not possible.

The next point is to show what has already been pretty clearly indicated, namely, that when the spare buoyancy and the moulded depth of a vessel are given, any stipulation as to her freeboard practically defines the form of the vessel. It defines within narrow limits that part of the displacement curve which is concerned with freeboard. Take, by way of example, moulded depth 26 feet, reserve buoyancy 30 per cent., and freeboards represented by 24 per cent., and 27 per cent of moulded depth. This difference of 3 per cent. is equal to nine inches, the difference of freeboard given by the table between the co-efficients ·60 and ·80. Enter the diagram on the horizontal lines corresponding to 24 and

to 27 per cent. of moulded depth, and trace them to where they intersect the vertical line representing 30 per cent. of reserve buoyancy, the points of intersection will be also points in the respective displacement curves of the two vessels. But we have already seen that these curves do not of necessity indicate co-efficients of fineness ·60 and ·80, as the mean curve in the figure will represent equally well either or both of these co-efficients. Is it not, then, quite correct to say that tables which connect percentage of buoyancy and moulded depth of a vessel with a fixed freeboard, thereby also define in a very important respect the model of the vessel?

If I have succeeded in establishing these two positions I shall also have proved that any tables which are founded on co-efficients of fineness are a delusion and a snare. With the assumed necessity for co-efficients of fineness must also vanish the need for the numerous columns of figures which are placed against them to represent freeboard. We thus arrive at the conclusion that the only terms which are really essential in tables of this kind are the moulded depth and the corresponding reserve buoyancy.

Granting, in the absence of a complete displacement scale or any ready means of calculating the freeboard which corresponds with any required percentage of reserve buoyancy, that it may be convenient to have a linear scale, I can offer an easily-remembered one for a vessel of medium form, which shall correspond to a nicety with the new Tables for steamers which have been issued by the Committee of Lloyd's Register: for these new Tables are purely lineal in character.

The old Liverpool scale for wooden vessels, as printed in our Transactions for 1870, page 44, may be briefly stated thus. A *fair* freeboard is an allowance commencing at 2·2 inches per foot for vessels of 10 feet depth of hold, and increases regularly by 0·1 inch per foot with each additional foot of depth. According to the new Tables by Lloyd's Register, a freeboard for iron steamers of medium form, which under no circumstances may be exceeded (as half an inch deeper would apparently render the ship technically unseaworthy), is as follows: 1·7 inches per foot for 10 feet moulded depth of vessel, and 0·1 inch per foot additional with each additional two feet in the moulded depth.

Shipowners and others who may wish to know what freeboard the new Rules issued by Lloyd's Register Committee will give for a flush-deck steamer, will find it convenient to remember this short formula.

After we have obtained the necessary displacement scales, there yet remains the inquiry, What is a suitable percentage of buoyancy, and who are the proper persons for deciding upon it? Where can this necessary information be found? The data for guiding our opinion on this subject can only be obtained by actual experience at sea. Unfortunately, these data are not usually given to us in the terms which we require. They have to be, as it were, translated into them. The duty of the translator will frequently be to accept the spirit while rejecting the literal meaning of what is put before

him. A great deal of apparently conflicting evidence has to be examined and generalised. The facts submitted have to be classed into various large divisions, and the bearing and relative weight of each must be estimated. Among other evidence, the first and foremost will be the experience of shipmasters, and of owners who have had the fewest casualties during a long period; the lengths, proportions, and models of their vessels; the trades in which they were engaged. There would also be the causes of deck damage, and the circumstances under which it occurs. There would also have to be considered the length and character of the waves inflicting the damage, compared with the length of the damaged vessel; the comparative frequency of the waves and weather which are found most trying to particular vessels; the height of freeboard at the time, and the relation of this height to the security of the crew in different vessels; the kind of deck fittings and erections; special construction of vessels for special trades;—possibly the season of the year during which certain voyages were usually made; the extra freeboard required for winter voyages in the Atlantic. These and a host of other details would have to be gone into, and the results would have to be harmonised and arranged before the exact and suitable percentage of spare buoyancy for any given vessel or class of vessels could be stated with confidence. It may be that what is termed "height of platform" in relation to length would, when fully considered, fix a minimum depth of freeboard, irrespective altogether of percentage of buoyancy, and that percentages of buoyancy would only have to be taken into consideration when they gave a greater freeboard than the minimum so obtained.

The subject of freeboard is confessedly a difficult one, even to those who have given a great deal of time and thought to it. For my own part, the more I follow it into its details, the more ready I am to adopt an axiom enunciated by a member of our Council, who will be acknowledged to be a great authority on the subject. This axiom will be found in the additions made to the report of the Royal Commission on tonnage, by the talented Secretary to Lloyd's Committee. It is as follows:—

"To correctly fix a maximum load-line it is necessary that there should be prepared for each vessel an accurate scale of displacement, from the keel to the gunwale."

With such a scale of displacement, and with a proper assessment of the tonnage above deck, all difficulty in cutting off any required percentage of reserve buoyancy will disappear. The only problems will then be the proper amount of reserve buoyancy for any particular voyage, or cargo, or season, or ship, and who are the proper persons to decide the question.

The unanimity of opinion which now prevails among shipowners in favour of a percentage of reserve buoyancy basis for fixing load lines, is most remarkable, and it is worthy of notice that Lloyd's Register Committee now give it their powerful support. This is the more remarkable as we have the most positive knowledge that at the close of

1875 they were totally opposed to it. Their reason for this opposition may be given in their own words, as follows :—

"To discuss the question of the proportion of surplus buoyancy suitable for all vessels," "would be taking a course that would inevitably tend to fixing a hard-and-fast load line." [See Parliamentary Paper, No. 8, 1876.]

Their present adhesion to the doctrine of reserve buoyancy for regulating freeboard may perhaps indicate opposition to the proposed appointment of a national shipping council for the settlement of this much debated subject. If the Committee of Lloyd's Registry saw fit to break up a useful council in 1875 for the reasons alleged, while in the midst of its deliberations—a council in which they appointed three delegates out of nine—can they be expected to look favourably upon the new council proposed by Mr. Chamberlain, in which their delegates may only number two out of a total of fourteen or seventeen ?

This paper will have failed in its chief object if it does not lead to the consideration of an improvement in the form of the official register for iron vessels. It is hoped that all will see the need of introducing into the register the "accurate scale of displacement from keel to gunwale," which Mr. Waymouth considers so necessary. Moulded depth will, I trust, be considered another measurement which should appear in the official document. Other changes are also needed. On the annexed copies of the official register form now in use, I have ventured, in the one, to underline all those entries which may be reasonably omitted, or altered and amended, at the present time: on the other, I insert proposed amendments which I trust will be able to speak for themselves, and will require no more advocacy on my part.

I have the pleasure of adding that one of the suggested improvements in the Official Register has already been made, as in some recent Registers I have observed the following additional and very important measurement :—"Depth from the bottom of keel to the top of deck next the waterway, measured amidships."

Official No. of Ship.	Name of Ship.	No., Date, and Port of Registry.

No., Date, and Port of previous Registry (if any).

Whether British or Foreign Built.	Whether a Sailing or Steam Ship, and if a Steam Ship how propelled.	Where Built.	When Built.	Name and Address of Builders.

		Feet.	Tenths.
No. of Decks ...	Length from fore part of Stem, under the bowsprit, to the aft side of the head of the stern post		
No. of Masts ...	Main breadth to outside of plank		
Rig ...	Depth in hold from tonnage deck to ceiling at midships		
Stern ...	Depth in hold from upper deck to ceiling at midships, in the case of three decks and upwards		
Build ...	Length of engine room, if any		
Galleries ...			
Head ...			
Framework ...			

PARTICULARS OF ENGINES (if any).

No. of Engines.	Description.	British or Foreign made.	When made.	Name and Address of Makers.	Diameter of Cylinders.	Length of Stroke.	No. of horses' power (combined).

PARTICULARS OF TONNAGE.

Gross Tonnage.	No. of Tons.	Deductions Allowed.	No. of Tons.
Under Tonnage Deck...		On account of space required for propelling power	
Closed-in spaces above the Tonnage Deck, if any...		On account of spaces occupied by Seamen or Apprentices, and appropriated to their use, and kept free from goods or stores of every kind, not being the personal property of the Crew	
Space or spaces between Deck		These spaces are the following, viz. :	
Poop			
Forecastle			
Round House			
Other closed-in spaces, if any, as follows : ..		Cubic Metres.	
Gross Tonnage			
Deductions as per contra			
Registered Tonnage		Total Deductions	

Name of Master..... Certificate of { Service, No. ..... / Competency, No.....

Names, Residence, and Description of the Owners, and Number of Sixty-four Shares held by each. } viz.,

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Dated ---------- Registrar ----------

Official No. of Ship.	Name of Ship.	No., Date, and Port of Registry.

No., Date, and Port of previous Registry (if any).

Whether British or Foreign Built.	Whether a Sailing or Steam Ship, and if a Steam Ship how propelled.	Where Built.	When Built.	Name and Address of Builders.

		Feet.	Tenths.
No. of Decks ..	Extreme Length		
No. of Masts ..	Length between perpendiculars		
	Extreme Breadth..		
	Breadth at Uppermost full Deck		
	*Moulded Depth and Depth of Keel, if any		
Material ..	Equivalent Depth for Round of Deck and Sheer		
	Length of Engine-room, if any		

PARTICULARS OF ENGINES (if any).

No. of Engines.	Description.	British or Foreign made.	When made	Name and Address of Makers.	Diameter of Cylinders.	Length of Stroke.	No. of horses' power (combined).

PARTICULARS OF DISPLACEMENT.

Feet Draught	Cubic Feet.	Cubic Metres.	Feet Draught	Cubic Feet.	Cubic Metres.	Feet Draught	Cubic Feet.	Cubic Metres.	Feet Draught	Cubic Feet.	Cubic Metres.
1			11			21			31		
2			12			22			32		
3			13			23			33		
4			14			24			34		
5			15			25			35		
6			16			26			36		
7			17			27			37		
8			18			28			38		
9			19			29			39		
10			20			30			40		
From the last recorded draught to the gunwale									..	.. ..	.. ..
For Round of Deck and Sheer									..	.. ..	.. ..
Total Displacement											

PARTICULARS OF TONNAGE.

Gross Tonnage.			No. of Tons.
Tonnage under uppermost deck			
Closed-in spaces above the uppermost full deck ..			
	Tons.	Assessment.	
1. Awning or shade deck.. ..			
2. Poop			
3. Quarter-deck			
4. Bridge			
5. Forecastle			
6. Hatchways			
Assessment			(here.)
Totals carried on to			
Gross Tonnage			
Deductions, as per contra			
Registered Tonnage			

Deductions Allowed.	Cubic Metres.	No. of Tons.
On account of space required for propelling power		
On account of spaces occupied by Seamen or Apprentices, and appropriated to their use, and kept free from goods or stores of every kind, not being the personal property of the Crew ..		
These spaces are the following, viz.:		
Total Deductions..		

Name of Master..... Certificate of { Service, No..... / Competency, No .....

Names, Residence, and Description of the Owners, and Number of Sixty-four Shares held by each, viz.,

.....

Dated..... Registrar .....

* A great improvement has recently been made in the official register of some vessels by the addition to it of the following measurement:—"Depth from the bottom of keel to the top of deck next the waterway, measured amidships."

DISCUSSION.

Mr. B. Martell: My Lord, I may say that I am personally so fairly tired of the discussion of the question of freeboard and surplus buoyancy, that I should not have risen to make any remarks upon this paper had it not been for some very serious observations which I think call for some explanation. Mr. Rundell states that in the tables of freeboard recently issued by the Committee of Lloyd's Register there are facts put forward in them that are inaccurate, and which must, consequently, be misleading. Now, that is a serious charge to make against a great public body like Lloyd's Register—that they should issue tables purporting to give certain freeboards corresponding practically with a definite percentage of surplus buoyancy, and which on examination are found to be perfectly inaccurate. Mr. Rundell says the facts put forward are inaccurate, and being so, of course they must be misleading. It would be very unfortunate for the shipping interest if that were so, because it is a matter of considerable importance at the present time, and therefore these remarks require some comment from me. I must say, I think that anyone who comes forward to make an assertion like that should take the trouble to ascertain, and be clearly satisfied in his own mind, that the facts he brings forward to controvert those put forward by a public body are in themselves accurate. Now, I should like to make a few remarks as to the criticisms which Mr. Rundell has placed before us for our consideration. Mr. Rundell refers to a paper that he read before this Institution in 1874, and in which were contained curves, not similar, but identically the same with the curves given in this paper now under discussion. And Mr. Rundell particularly draws our attention to that fact. Well, now there is a very singular anomaly in connection with that. In the paper read by Mr. Rundell in 1874 he states distinctly that the depth of ships taken by him in the cases then given was taken from the bottom of the keel when there was a hanging keel, and when there was not, it would be taken from the flat plate keel. He also says in that paper that he does not take cognizance of a very important element, the sheer of the ship, neither does he include the round of the beam in those ships. In the present paper he tells us that he does in this instance take notice of the sheer and of the round of beam, and he makes some kind of addition for this. I do not know what allowance there is, but some addition is made at the top of the curves in some way to meet this. But there is this very important difference, irrespective of this, that in the present case the depth taken is the *moulded* depth of the ship instead of the depth to the bottom of the keel. Now, it is perfectly impossible, I conceive, that you can produce curves like these, perfectly agreeing with each other as to surplus buoyancy and displacement of ships, as Mr. Rundell states they do, and yet that you should take the one from the bottom of the keel, and the other you should take from the *moulded* depth of the ship. Therefore, unless some further explanation is given, it will be impossible for me to accept these curves as being in any kind of way accurate. Neither is it explained anywhere how these co-efficients are obtained—no explanation is given, and no one can tell how they are obtained. Mr. Rundell again says he produces curves of twenty-five ships: I do not know how the curves representing these ships are formed, or how they are plotted on the diagram shown, because he does not tell us in any way; and until we can ascertain more about these particulars it is perfectly impossible to express any opinion whatever on their accuracy, because there is evidently a very great and important anomaly in the comparison attempted to be set up. I more particularly protest against the accuracy of the curves shown here in view of, not twenty-five ships, but of the very large number of actual ships we have taken, of which we have had the drawings supplied and the calculations made in our office; and those drawings show from the calculations made, that in the tables of freeboard which were framed in Lloyd's Register office, the

freeboard and surplus buoyancy practically conform with each other. We have, I may say, received from some shipowners a statement that our freeboard does not correspond with the surplus buoyancy given. When that has been the case, we have said, "Well, if you will furnish us with the drawings of the ships we shall be happy to take the trouble of making calculations ourselves so as to ascertain if it is so, and to see if you are correct in what you say." We have done so in many instances, and in *every* instance it has been found that there has not been a greater difference than about one-tenth of one per cent. in the surplus buoyancy given in those Tables and in the freeboard corresponding with the same. If, for instance, we were to take 30 per cent. as the surplus buoyancy, the difference from the actual calculation made from the drawings supplied to us and that given in the Tables—not one, two, or three, but the whole number given to us by shipowners—has been 30·1 and 30·2, practically making no difference between the Tables and the actual calculations. Mr. Rundell, if he makes inquiry from some of the most eminent shipowners in Liverpool who sent in drawings in that way, will find what I say to be correct. Therefore, it is impossible for me to accept what Mr. Rundell gives as being correct illustrations of the various displacements and surplus buoyancy of ships, unless the names of the ships are given. This must be given, and we must be allowed to make the calculations ourselves before we can accept statements of this kind. As regards the supposition that the upper two lines in the tables of surplus buoyancy and moulded depth only are necessary, the fact is lost sight of that the thousands of ships in existence have to be provided for, without the loss of time and expense in docking them and preparing drawings and calculations, and the Tables, I assert, enable this to be done with practical accuracy. Mr. Rundell, with very great inconsistency—I cannot help saying so, although I am very sorry to be obliged to do so—refers to his paper of 1874, where he shows us there is a very easy method of cutting off 30 per cent. of surplus buoyancy by taking 25 per cent. of the moulded depth, and yet nearly the whole of this paper of his is taken up in proving that Lloyd's cannot do it, and that he cannot do it, and that it is an impossible thing to do. That does not seem to me, I must say, to be very consistent. Then as to this triangular prism shown on the diagram, I cannot see what it has to do with this matter at all, and I should like Mr. Rundell to explain it. If we had ships that floated on their decks sometimes and at other times with their keels downwards, I could understand the meaning of it, but we have to deal with actual ships, and unless you deal with actual ships you are doing nothing with an important practical question of this kind. Then, again, if you were to "regularly increase or decrease the areas of every water-line from the deck to the keel in the same proportion referred to," what does that show? It is not a part of the subject, as every professional shipbuilder here knows. Suppose you want to make a ship finer or fuller, with a certain breadth and form of deck, and you wanted to make her finer, do you think a man would reduce the breadth of the deck of the ship? Certainly not: he would fine it from the load line downwards—not increase or diminish the ship all over, as supposed by Mr. Rundell; if, however, he did, it would alter the thing altogether, because you would get a different size and a different form of ship, with a different displacement and different co-efficient. I may say that Lloyd's Tables have been prepared—(and they have taken months)—with the very greatest care, and the greatest desire to arrive at correct results, and I think, at least, that Mr. Rundell will be willing to give credit to those engaged in Lloyd's Register Office for knowing at least sufficient of their business to be able to calculate displacements of vessels, and to know how to arrive at results with regard to actual facts such as he alludes to; and, therefore, if we have misrepresented these and put them forward in these Tables, it must be for a purpose, and that purpose is one that is highly to be condemned: a conclusion at which I need not say I protest against, as at all applicable in this case. I should like Mr. Rundell, when he is making a statement to this effect, to go further and to *prove* that we have

done it, before he comes before the public to make such an assertion, deduced from a drawing of the kind before us, of which members of this Institution have no means of comparing with ships and ascertaining the correctness. I can only add that although the greatest care has been taken in the preparation of these Tables, they are, of course, open to modification, if experience shows it to be necessary; but it will not be on such evidence as that brought forward in this paper, I am certain, that Lloyd's Register Committee will have to alter these Tables, after the time and attention which have been given in getting out these results for the guidance of those interested in the subject.

Mr. RUNDELL: My Lord, a doubt has been cast upon the identity of the tables, or rather the diagrams, published in our Transactions of 1874 and the present. I confess that they are not precisely identical, there is just this difference: you measure a man first perhaps in his naked feet and then in his stockings, or perhaps with a cap on his head; if you then express the height of some part of his body as a percentage of total height, there may of course be a small difference of percentage in the two cases, but will it be an appreciable difference? It is really absurd to point to any matter of that sort as materially affecting the similarity of the two diagrams.

Mr. MARTELL: May I just explain, my Lord? I should like to say with reference to this that when I say that this difference amounts to between 300 and 400 tons displacement in the largest ships, I do not call it a little thing.

Mr. RUNDELL: In speaking of quantities and percentages, the percentage may be the same, or very nearly the same, although the quantities may vary very much. The advantage of taking percentages is that you eliminate all these points of difference and enable yourself to compare apparently different things on the same basis. As regards the co-efficients of fineness, I think I have on the diagram explained exactly how they are obtained. It says on the diagram that "the curves of displacement between the lines A, B, and C, are for actual vessels; the figures indicate the co-efficients of fineness as calculated from the registered dimensions, and the registered under deck tonnage;" so that this point, which is supposed to be absent, you will see is included in the paper. It seems to me, my Lord, that I have had a problem put before me now that I did not contemplate when I began this paper. I tried to explain and to give my reasons in a manner which seemed to me to be perfectly conclusive, but of course I did not undertake that Mr. Martell should understand those reasons or agree with them. That was no part of the contract. I have to find the arguments but I have not to make Mr. Martell understand them. That was certainly a thing I never contemplated; and, besides, my Lord, there is one thing which would prevent me from replying even to an apparently fair argument, and that is, if it were advanced with a sneer. Allow me to say that a sneer is not an argument.

The PRESIDENT: Gentlemen, I am sure you will allow me to return your thanks to Mr. Rundell for his very valuable paper. We all know of the very long experience he has had and the great interest he has taken in these questions, and I am sure you will allow me to thank him for the paper he has read. With regard to the discussion that has taken place, we all know that it is only by the conflict of intelligence, and intellect, and experience, that the truth is arrived at; therefore, when we have conflicts between Mr. Martell and Mr. Rundell we always expect, at any rate, that we shall get at the truth at last; how long it will be before we arrive at it I cannot say.

ON THE ASSESSMENT OF DECK ERECTIONS IN RELATION TO FREEBOARD.

By Henry H. West, Chief Surveyor, Underwriters' Registry for Iron Vessels; Member of Council.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

What is the object of Freeboard? What are its functions?

Perhaps the most important of these are: To limit the ship's load; to provide a reserve of buoyancy, both as a margin against leakage and as lifting power in a sea way; to assist in securing a sufficient range of stability; to provide a suitable height of working platform, and to protect the vessel from deck damage.

When we consider these various functions, and how the conditions which call them into play vary both in degree and in combination, we shall, I think, be ready to admit that the instinct which has always led the experienced and self-reliant shipowner to protest against any fixed load line, is a true one. He knows how apt a so-called "fair load line" is to crystallise into a "hard and fast line," the enforcement of which would seriously cripple him in his business; and how, on the other hand, an authoritative "maximum load line" would too often be claimed as a legalised draught, and even be used, ignorantly, under conditions which would court disaster.

An authority that we all respect, and whose deliberations have ever aimed at conserving the best interests of the shipping community, has taken a different view from this. The officers of Lloyd's Register have most perseveringly attacked the problem of a load line, and after several *quasi* public and more or less successful attempts, have at last issued an official manifesto on the subject. In a series of very carefully adjusted Tables, with an elaborate preface, they have given a load line for almost every conceivable ship which has been or which can be built. It will be interesting to trace whether, and at what stage in their history, these Tables have secured any important support from the great body of shipowners.

We have it on the authority of the compiler of the Tables upon which the amended and re-amended Tables have been founded, that they were originally framed on the

practice of shipowners "you would like to follow:" * and I know no safer guide, in such a matter, than the experience of those owners whose reputation for good management, and whose immunity from accident, have justified their practice. But how was this formulating of the good practice of the day then received? For answer I quote from the Final Report of the Royal Commission on Unseaworthy Ships:—"Witnesses who have examined this scheme say that if such a test were applied and enforced, ships which have hitherto made their voyages in perfect safety could no longer be profitably employed."

After some important alterations the Tables were again put forward, and in the early part of last year were widely circulated for criticism. Our own Transactions will show how far they still were from receiving unanimous approval. But the last issue—the re-amended Tables—the final edition, clothed with all the authority of official sanction, has attained a fair degree of popularity.

Let us see if we can find any clue to this appreciation in Lloyd's Tables themselves.

TABLE A.

Depth. Moulded. Ft. In.	Depth. Hold. Ft. In.	Freeboard in Inches by Tables of October, 1873.	Freeboard in Inches by Tables of March, 1882.	Freeboard in Inches by Tables of August, 1882.	Difference in Inches between Tables of October, 1873, and March, 1882.	Percentage of Reduction.	Difference in Inches between Tables of March, 1882, and August, 1882.	Percentage of Reduction.	Difference in Inches between Tables of October, 1873, and August, 1882.	Percentage of Reduction.
10·0	9·3	19·75	19·0	16·5	0·75	3· 7	2·5	13·15	3·25	16·45
15·0	14·0	32·5	31·0	28·5	1·5	4·61	2·5	8·06	4·0	12·30
20·0	18·9	50·5	45·0	43·5	5·5	10·89	1·5	3·33	7·0	13·86
25·0	23·6	72·25	64·5	61·0	7·75	10·72	3·5	5·42	11·25	15·57
30·0	28·3	103·0	91·5	80·0	11·5	11·16	11·5	12·56	23·0	22·33

TABLE B.

	Freeboard in Inches by Tables of October, 1873.	Freeboard in Inches by Tables of March, 1882.	Freeboard in Inches by Tables of August, 1882.	Difference in Inches between Tables of October, 1873, and March, 1882.	Percentage of Reduction.	Difference in Inches between Tables of March, 1882, and August, 1882.	Percentage of Reduction.	Difference in Inches between Tables of October, 1873, and August, 1882.	Percentage of Reduction.
By table	53·25	46·5	45·0	6·75	12·67	1·5	3·22	8·25	15·49
Less deduction for erections ...	3·55	9·90	12·3	—	—	—	—	—	—
Final freeboard...	49·7	36·6	32·7	13·1	26·35	3·9	10·65	17·0	34·2

* Mr. Martell's evidence before the Royal Commission on Unseaworthy Ships: Question 11,208.

In Table A, I have grouped together for comparison the freeboards assigned by Lloyd's Tables of October, 1873; March, 1882; and August, 1882, for vessels of medium form (co-efficient ·74) of 10, 15, 20, 25, and 30 feet moulded depth. I have also shown the difference between the freeboards of 1873 and 1882 in columns of inches and of percentages. Again, in another table (B), I have given the same particulars for a screw steamer, 260 ft. × 34 ft. × 18 ft. 9 in. hold, and 20 ft. moulded depth, with ·78 co-efficient of fineness, having a long raised quarter-deck 3 ft. 6 in. high, with bridge-house and forecastle, the combined lengths of which amount to seven-tenths of the vessel's length.

It will be seen that in these illustrative cases, the freeboard has been reduced, between October, 1873, and August, 1882, in no instance by less than 12 per cent., and in one case by more than 34 per cent. These figures show that the practice of shipowners one "would like to follow" has been pared down by no flinching hand. I cannot blame shipowners, if, under the harassing pressure of recent legislation, they are ready to welcome a load line which bears the honoured name of Lloyd's, and which, if necessary, may be made to justify even a somewhat extreme practice.

But perhaps I shall be told that these freeboards give *maximum* load lines. So be it. I understand this to mean the deepest immersion to which the ship may be loaded under any circumstances whatever. A short voyage, on placid waters, at their most placid season; a captain ready for any emergency; a thoroughly disciplined crew, men who have a well-justified confidence in each other and in their ship; a cargo combining those happy proportions of weight and measurement, and those rare opportunities for their nice relative adjustment, which help to make perfect stowage;—these are the conditions to which I understand a *maximum* load line to apply.

But we are also told the freeboard for vessels crossing the North Atlantic in the winter months,—one of the worst voyages that the world knows. What do we find to be the difference of freeboard assigned by the Tables to these widely differing conditions? A matter of a few inches only. In the very largest flush-decked vessels which the Tables contemplate, it amounts to less than nine inches, and in the smaller type of steamers which would brave an Atlantic winter, three inches is the whole difference. Are both these extremes right? It is a question I cannot venture to answer; but I draw attention to it, not in any spirit of hostility to Lloyd's, but to illustrate the extreme difficulty, not to say danger, of attempting to fix a load line, whether, like Lloyd's line of 1873, it passes under the name of "a fair line of reference for ordinary trades," or, like Lloyd's line of 1882, it bears the name of "a maximum load line in salt water."

Moreover, tables of freeboard for flush-decked vessels can have only a very limited practical application without some scheme of allowances for deck erections, based on an equitable assessment of their value in relation to freeboard. It is in this direction, rather than in attempting to fix load lines, that I believe the Classification Societies may render the most valuable assistance, not only to the shipowner, but also to the under-

writer, the shipper, and the general public, to whom, let me say, they owe a duty as real as that which they owe to the shipowner.

I take it that there is now a universal consensus of opinion that a prime element of proper loading is that some given proportion of the whole bulk of the ship, varying according to varying conditions, shall remain above water as a reserve. Further, that this reserve may be partly below the main deck, and partly above it, in substantial erections forming structural portions of the ship; such as spar-decks, awning-decks, raised quarter-decks, poops, closed in bridges or forecastles. It is equally accepted that such erections have varying values, according to relative size, strength, and position. Again, it is also agreed that, apart altogether from the amount of reserve bulk, a sufficient height of working platform, varying according to varying conditions, should be maintained. But while there is so much general agreement, there seems to be the widest divergence of opinion as to the relative value of erections above deck as compared with that portion of the reserve which is under deck. Here, then, is a problem, the fair assessment of deck erections, the solution of which will dispose of many, if not of most, of the grounds of dispute in cases of assumed overloading; and it is one which the Classification Societies are eminently fitted to solve. They have an intimate knowledge of the arrangements and structural strength of their classed vessels, they have a trained staff, and, with the co-operation of builders and owners, they have the means of preparing authentic displacement scales for every classed ship, and they usually have the confidence of all the interests concerned.

The Surveyors of the Underwriters' Registry for Iron Vessels have given a good deal of attention to this problem, and I now submit for your consideration and criticism the conclusions at which we have arrived.

In approaching the question, we started with the intention of stating for any vessel the mean draughts which would, with practical accuracy, immerse certain definite percentages of the whole volume of the vessel, or their equivalents. The percentages decided upon were 65, 70, 75, and 80 per cent., corresponding to reserves of 35, 30, 25, and 20 per cent. respectively. These figures were not intended to indicate load lines, but simply to show the lines of immersion at which such percentages would be reached; and were adopted because the Committee of the Underwriters' Registry, having determined to give complete displacement scales on their certificates of class, decided specially to mark the mean draughts which corresponded to these percentages.

It is obvious that to do this for a flush-decked vessel is a matter which simply involves a calculation of the whole volume of the ship, including sheer, round of beam, and thickness of deck; and the preparation of a displacement scale from which the specified percentages, or any others, can be read off.

But when to this displacement under deck had to be added an assessed value for the displacement above deck, the matter became more complex, and it was necessary to discuss

some general principles, and frame some general rules, which should be applicable to all the cases contemplated.

It will be convenient here to state, that in speaking of displacement under deck I mean the displacement (external measure) of the whole vessel below the upper surface of the deck, including sheer, round, and thickness of the deck upon which the erections stand; and by displacement above deck, I mean the sum of the displacements (external measure) of the erections above deck. For convenience of calculation, and for other reasons with which I need not now trouble you, we adopted for our unit of displacement a ton of 100 cubic feet.

We decided that the assessment of deck erections should be in terms of under deck displacement, that is to say, that each unit of displacement above deck should be assessed as some fraction of the same unit under deck. We will call this fraction the co-efficient of assessment.

It appeared to us self-evident that the value of this co efficient must vary in some proportion to the strength and efficient closing in of the structure under assessment.

We also accepted it as a postulate, that the co-efficient of assessment must vary in some proportion with the ratio which the displacement of the ship under deck bears to the displacement of the erections above deck. This is, perhaps, not so obvious, but if we put it in another form it will be clearer. The value of large erections on a small ship is relatively less—is less ton for ton—than the value of small erections on a large ship. Thus, putting T for the sum of the displacements of the erections on deck, and $\mathfrak{T}$ for the displacement under deck, the fraction $\frac{T}{\mathfrak{T}}$ in some form would appear in the formula for the co-efficient of assessment, the value of the co-efficient being greatest when that fraction was least.

Again, it appeared clear that the value of a low erection is—ton for ton—greater than the value of a high one. Taking the height of an ordinary spar-deck as a standard of comparison, and putting H for this height, and h for the height of the particular erection under assessment, the fraction $\frac{h}{H}$ would also appear in some form as a factor in the equation for the co-efficient of assessment, the value again being greatest when that fraction was least.

Another element yet remained to be represented. It appeared to us that the value of an erection in relation to reserve buoyancy would be greatest when that erection was arranged symmetrically about the middle of the vessel's length; so that, other things being equal, a ton in an erection extending from end to end of the vessel, would have a greater value than a ton in an erection extending from aft to amidships only; or a ton in an amidships erection would have a greater value than a ton in an erection at one of the ends of the vessel. Putting d for the distance from the centre of the vessel's length to the centre of length of the erection under assessment, and l for the half length of the vessel, the fraction $\frac{d}{l}$ in some form would also appear as a factor in the equation for the co-efficient of assessment, the value again being greatest when that fraction was least.

We thus obtained the following equation for the value of the co-efficient of assessment C:—

$$S \times R \times r \times p = C.$$

Where S = The structural element; that part of the value affected by strength and efficient closing in.

R = The ratio of displacement, involving, as we have seen, the fraction $\frac{T}{\mathfrak{T}}$.

r = The ratio of height, involving the fraction $\frac{h}{H}$.

p = The element of position, involving the fraction $\frac{d}{l}$.

In assigning values to these factors it was seen that the following consideration must, in each case, necessarily underlie the valuation. That in an erection in the form of a continuous closed in deck, all fore-and-aft, of full scantling strength, every ton of displacement in that erection is entitled to rank on equal terms with a ton of displacement under deck. Hence the value of C in that case must necessarily be unity; and, since the value of each factor, S, R, r and p, can never be greater than unity, it follows that the value of each must, in the case under consideration, be unity. Therefore, the value of S must either be 1 or some fraction less than 1, according to the strength and structural efficiency of the erection. And since the value of R must be 1 when S=1, and, as we have seen, the value of C must be greatest when $\frac{T}{\mathfrak{T}}$ is least, the value of R will be some form of the expression, 1 minus such form of the fraction $\frac{T}{\mathfrak{T}}$ as will vary with the value of S, and disappear when the value of S rises to 1. In the same way, the value of r will be some form of the expression, 1 minus such form of the fraction $\frac{h}{H}$ as will also vary with the value of S, and disappear when S rises to 1. With reference to the factor p, the conditions are that its value, when the centre of length of the erection and the centre of length of the vessel coincide—that is to say, when $d=0$, the value of p shall be 1, and when $d=l$ the value of p shall be 0. All these conditions are fulfilled by the formula:—

$$S \times \sqrt{1 - \frac{T-ST}{\mathfrak{T}}} \times \sqrt{1 - \frac{h-Sh}{H}} \times \sqrt{1 - \frac{d}{l}} = C.$$

Having thus obtained a co-efficient of assessment, if we put t for the displacement of the particular erection to be assessed, Ct will be, in terms of displacement under deck, the assessed value of that erection; which value, added to the displacement under deck, gives the total assessed displacement. Of this total the displacement scale gives the means of at once drawing the various percentage lines.

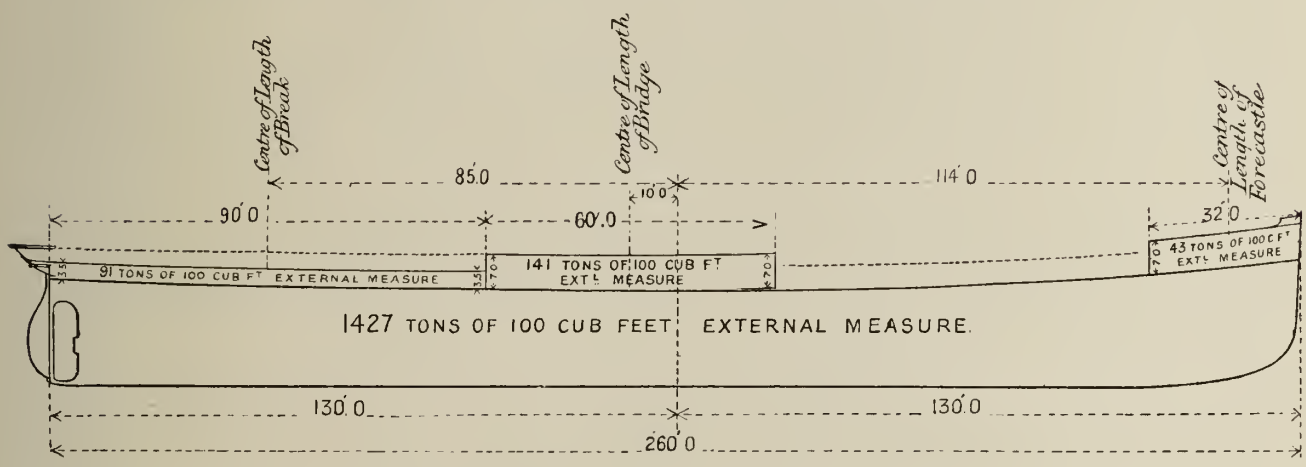
This method gives us an assessment which takes into account actual and relative capacity of the erection, its strength, its height, and its position; and is governed by the actual facts of each case, without any room for individual opinion, except in the choice of the factor for strength and structural efficiency. This would be the province of the surveyor. He would choose what factor between 1 and 0 was applicable to the particular case, and he would be guided in his choice by the strength of the erection, by the number, kind,

and position of openings in it, and by a comparison of its scantlings with the published Rules of the Classification Society. But his choice once made, the rest is a mere matter of arithmetic. Even this small margin for difference of opinion might be minimised if the Classification Society would decide the value for S for average cases of each type of erection. Any departure from this value, in either direction, would then have to be justified to, and confirmed by, the Classification Committee.

In the Appendix to this paper will be found an example of a break-deck vessel with the assessment of the deck erections worked out in detail, from which it will be seen how simply and readily this method may be applied. The resulting draughts might, at the owner's request, be published in the Register Book. Thus the owner, the underwriter, the merchant, or whoever might have an interest in the matter, would, on fully stated lines of procedure, be provided by an unbiassed authority with a means of easily ascertaining what percentage of any vessel was immersed at a given draught; and at the same time the owner would be left absolutely free to adopt any load line that circumstances, and the mutual consent of his co-adventurers, might justify.

APPENDIX.

Screw steamer :—260 ft. × 34 ft. × 18·75 ft. = 1,279 tons register under deck.



$$\text{Formula :—} S \times \sqrt{1 - \frac{T - ST}{\mathfrak{T}}} \times \sqrt{1 - \frac{h - Sh}{H}} \times \sqrt{1 - \frac{d}{l}} = C.$$

Displacement under upper surface of deck, including sheer and round of beam = 1427 tons = $\mathfrak{T}$

t =	Displacement of break (external measure)	... ... =	91 ,,
t =	,, ,, bridge (external measure)	... ... =	141 ,,
t =	,, ,, forecastle (external measure)	... ... =	43 ,,
	Sum of displacements of erections above deck	... ...	275 = T

Height of break	3·5 feet	$= h$
,, ordinary spar deck	7·0 ,,	$= H$
Distance from centre of vessel's length to centre of break	85·0 ,,	$= d$
Half vessel's length	130·0 ,,	$= l$
Assuming that the structural efficiency for break is fixed by the Surveyor at...	0·6 ,,	$= S$

Then assessed value of break $= Ct$

$$= 0{\cdot}6 \times \sqrt{1 - \frac{275 - (0{\cdot}6 \times 275)}{1427}} \times \sqrt{1 - \frac{3{\cdot}5 - (0{\cdot}6 \times 3{\cdot}5)}{7}} \times \sqrt{1 - \frac{85}{130}} \times 91 = 0{\cdot}3 \times 91 = 27{\cdot}3 \text{ tons} = Ct.$$

Height of bridge	7·0 feet	$= h_1$
,, ordinary spar deck	7·0 ,,	$= H_1$
Distance from centre of vessel's length to centre of bridge	10·0 ,,	$= d_1$
Half vessel's length	130·0 ,,	$= l_1$
Assuming that the structural efficiency for bridge is fixed by the Surveyor at	0·5	$= S_1$

Then assessed value of bridge $= C_1 t_1$

$$= 0{\cdot}5 \times \sqrt{1 - \frac{275 - (0{\cdot}5 \times 275)}{1427}} \times \sqrt{1 - \frac{7 - (0{\cdot}5 \times 7)}{7}} \times \sqrt{1 - \frac{10}{130}} \times 141$$

$$= 0{\cdot}322 \times 141 = 45 \text{ tons} = C_1 t_1.$$

Height of forecastle	7·0 feet	$= h_2$
,, ordinary spar deck	7·0 ,,	$= H_2$
Distance from centre of vessel's length to centre of forecastle	114·0 ,,	$= d_2$
Half vessel's length	130·0 ,,	$= l_2$
Assuming that the structural efficiency for forecastle is fixed by the Surveyor at	0·4	$= S_2$

Then assessed value of forecastle $= C_2 t_2$

$$= 0{\cdot}4 \times \sqrt{1 - \frac{275 - (0{\cdot}4 \times 275)}{1427}} \times \sqrt{1 - \frac{7 - (0{\cdot}4 \times 7}{7}} \times \sqrt{1 - \frac{114}{130}} \times 43$$

$$= 0{\cdot}083 \times 43 = 3{\cdot}56 \text{ tons} = C_2 t_2.$$

Summary.

Displacement under upper surface of deck, including sheer and round of beam, in tons of 100 cubic feet	= 1427·00
Assessed displacement of break	= 27·30
,, ,, bridge	= 45·00
,, ,, forecastle	= 3·56
Total assessed displacement in tons of 100 cubic feet	= 1502·86

65 per cent. of total assessed displacement	= 977 tons	= 15.05 feet mean draft.	
70 ,, ,, ,,	= 1052 ,,	= 16·07	,,
75 ,, ,, ,,	= 1127 ,,	= 17·09	,,
80 ,, ,, ,,	= 1202 ,,	= 18·11	,,

DISCUSSION.

Vice-Admiral De Horsey: My Lord, in considering the question of free-board and of allowance for deck erection, I would wish to point out that a deck erection, in my humble opinion, could not be considered as justifying a smaller amount of freeboard. A ship is nothing but a box with a lid on. Anything you put above that lid in the shape of a deck erection does not come into play to save that ship from sinking until the deck erection comes down to the water's edge. When that extreme case occurs, who will say that a deck erection will save a ship? Where do you find a ship with deck erections which are water-tight, which will keep the water out in that extreme case which may occur after a collision, when the deck erection comes near the water? It is for that reason that I am strongly of opinion that we should not take it into account in assessing freeboard. I also wish to express my opinion that the owners and captains of ships are the best judges of where their maximum load line should be. No rule the Government can make will apply satisfactorily to ships of different type and different age, to different seasons and different owners and captains. I think it is most misleading to have an established fixed load line. I apprehend it is more likely to lead to losses than to save them. It will be taken advantage of by those who go to sea in a ship otherwise unseaworthy, and they would say, "Here is my line in accordance with the Government mark and Lloyd's Register; I was right in putting it there; if the ship sinks it is not my fault." I would far rather leave the responsibility with them. Let them put their maximum load line where they think right, and don't make a hard and fast load line that must be deceptive.

Mr. W. H. White: I do not rise for the purpose of speaking, but Mr. Withy last year spoke on Mr. Rundell's paper which was then partially read. I know that he takes great interest in those ships in which there are erections above the flush deck, erections which he considers of great value; and I think if he would give us his opinion as regards this method of assessment as affecting them, it would be of great interest to the Institution.

Mr. E. Withy: My Lord, I have certainly taken a great deal of interest in this question, and I have found there is a great amount of misapprehension as to what the deck erections are, which we on the North-East coast consider to be valuable. The gallant Admiral evidently does not understand what they are at all. In the first place, these deck erections extend to the sides of the ship; the main frames of the ship run through them; the plating is carried up; the decks are solid on the top of them, and as water-tight as the main deck itself; the ends are closed in by strong water-tight iron bulk-heads; the entrance to all of them, except in the case of the top-gallant forecastle, is from the top, and those openings are now mostly protected by strong iron hatches or strong iron companions. It is a very general thing now in cargo boats to make even the skylight and companion of the engine-room and cabin entirely of iron. That these erections do not come into play as surplus buoyancy until the vessel has immersed her main deck is to a certain extent true, but the same may be said of the out-of-water portion of the vessel above the load line and under the main deck. Until the vessel begins to sink inch by inch, none of the parts out of water come into play. Therefore, if a deck erection is not to be considered as surplus buoyancy until you have immersed the main deck under it, the same applies exactly to any portion of freeboard of a flush deck vessel. My own opinion about this question of surplus buoyancy is that it is not a thing to speak of absolutely. It is a relative question. It is a measure which enables us to form some reasonable kind of opinion as to whether the ship has enough out of water to enable her to lift to the waves. I

do not think it is more than that, for the reason that I have just given, that none of it comes into play until the vessel is immersed beyond her ordinary loading. During the last week I sent four very long letters to the *Shipping Gazette*, confuting as far as I could a paper written in the February number of the *Nautical Magazine* signed by "Carabos." I have also in the last letter analysed in rather a sketchy way the present Board of Trade Rules. These Rules were for a long time kept in a pigeon-hole, but at last they have been brought to light. We have also an explanation, in the form of a paper by Sir Digby Murray, showing on what basis he formed those Rules. I have criticised them in as fair a spirit as I could, not wishing at all to raise any kind of animosity, and not wishing to put forward any ideas as especially my own. But I may say that it fills me with amazement that a nation like ours can be governed in an important interest like this, in its largest and most national trade —I hardly know how to put it in mild language, really—by such perfectly absurd treatment. Now I agree to a certain extent with those who say that the shipowner and the captain are the proper people to fix the loading. It is very easy to say that that has gone on, or that it did go on up to the time of Parliament taking it up some eight or nine or ten years ago. What was the result? The result was that owners and captains who were not high-principled men overloaded their ships most shockingly. I have gone myself, before the Plimsoll agitation, to the collector of customs in our own port and called his attention to a ship which I said was scandalously overladen. I felt that I dared not go home and leave the case without a protest. I never saw a more scandalous instance than the one to which I allude, but there was no means of taking official cognizance of it. I have known a vessel to be loaded with fully 11 per cent. more dead weight than we had sold her to carry. That was in the old days. Now, how are you going to stop that unless you apply some check? The law, we are told, is "for the terror of evil-doers and for the praise of them that do well," and I can see no practical outcome of all this discussion without we get a load line. Then, I say, let us get the very best load line we can. In the concluding letter that I sent to the *Shipping Gazette*, I appeal to the Board of Trade and to shipowners that they should adopt, tentatively, Lloyd's assessment. I do not consider that it is perfection, but am certainly not inclined, without looking more carefully over Mr. Rundell's paper, to think that the question of co-efficients of fineness can be entirely eliminated without a great deal of harm. I know we often build different ships of the same dimensions in which one may carry from 150 to 200 tons more than the other. If a ship has such an extra weight to carry, and has to lift it over the waves she approaches, and has to rise to those that follow, she must have an additional amount of—whatever you like to call it; I have called it in one of my letters, "outside surface." I have called attention in one of these letters to this: that if we take an open boat that is perfectly straight on the top, and another that has a good rise of sheer at the fore and after ends, then as the latter approaches the waves they rise up over the outside surface and will lift her in proportion to the outside surface she has. The next consideration we come to is what will happen to the waves that come over the top. In a decked vessel they will get off again. If the vessel is undecked they will, of course, remain at the bottom. Then comes the question, which Mr. West has brought before us so well to-day, of deck erections. A great deal has been said against what has been ordinarily called "well-decked" vessels as we supply them principally on the north-east coast. I have taken more interest in them because I have found them most severely attacked. It is my own view, that if you have a "flush-deck" vessel with a bulwark all round you have a "well" the whole length of the ship. If you put a top-gallant forecastle on her you take off some 30 or 40 feet from the length of the well with the deck on it level with the top of its rail. Therefore whatever water is thrown on that runs off again. If you raise the middle of the vessel (and I am glad to see that Mr. West lays very great stress upon the value of that) by putting a bridge deck with strong bulk-

heads at the front and at the after end, and raise the engine and boiler openings, and put down all the further necessities of navigation—the steering gear, the standard and steering compasses, the telegraph to the engine-room, and above all the combings of the hatches that lead down into the stoke-hole—which a sea entering may put the fires out—then you enormously improve the vessel, and reduce to that extent the amount of water that can be lodged upon her deck. If, again, you raise the after end of the vessel in the form of a raised quarter-deck and join that to the bridge-house, you again reduce the "well." I was asked at a recent Board of Trade inquiry how "wells" came to be introduced. I said they were not introduced, and drew the attention of the Court to the fact that if they went back about fifteen or twenty years they would remember that nearly all steamers had a flush deck and a close bulwark, most of them having a top-gallant bulwark above that, and a monkey forecastle for working anchors and coiling warps on and so forth; that their engine and boiler hatches were very low, and yet were surrounded by this high bulwark; that at the after end they possibly had a little quarter-deck of about thirty feet long, two or three feet above the main deck, in which the cabin was placed. I can remember when I was an apprentice that we built that class of ship, and we used to put a companion to go down into the cabin at the fore end of the quarter-deck with its door cut down through the bulkhead at the front. I think it was about a foot above the main-deck. There could be nothing more beautiful as a conductor of water into the cabin (unless you go to a funnel which is put into the mouth of a cask to pour water in) than that kind of companion, but it was regularly tolerated at the time. Having such a vessel as that, which I think all present will admit was the kind in vogue fifteen or twenty years ago, to start with, we first of all continued the quarter-deck further on towards amidships. Then we extended the bridge-house aft, and the two were joined; but still the front of the bridge was left open, and there were passages at each side extending right up to the quarter-deck, so that you could walk along them and a very few steps led up to the quarter-deck. It was very soon found that these passages were really water traps, and after we had tried all sorts of schemes to let out the water that came in, by means of waterport doors worked with levers, and so on, we came to the conclusion that we must shut up the front, and then we did so. In the next place, we put a top-gallant forecastle on. Everyone will admit that a vessel with a top-gallant forecastle takes very much less water on board and rises very much better to the approaching waves than a ship without. Now you have arrived at the modern "well-decker." It was not that we adopted the "well," but that at every step we took we enclosed a part of the "well" that was there before. I might, I think, repeat what I said here last year, that ships that are called "well-deckers" have less "well" than any other class of vessel having close bulwarks. I have put into one of these letters to the *Shipping Gazette* a challenge in this way, as it seems to me that very many people, when they speak of "well-deckers," mean any ships with close bulwarks. I say let us fight out the battle on that issue—*close* bulwarks or *open* bulwarks. That will be quite a distinct issue. I have assumed, too, in this letter that it cannot be the poop and bridge and forecastle, or the quarter-deck, or such buoyant and water-tight deck erections that our opponents object to, therefore it must be the bulwark, and if so, let us have it in defined terms. In this month's *Nautical Magazine* there is a letter published as to the losses of "well-decked" ships; it is signed by "A Nautical Assessor to the Board of Trade." In it he gives a list which he says he has before him of ten of these vessels which have been lost, and 147 lives with them. I have analysed that statement, and find one of the vessels is a sailing ship, three of them are three-deck cargo boats, which are flush-decked vessels, many of them having poop, bridge, and forecastle; the type of one of them I cannot make out. That disposes of half of the list of ten so called "well-deckers." There are only four or five "well-deckers" in the whole of the list. Now this shows how exceedingly loose many of our opponents are in the way in which they attack these

vessels. I mentioned here last year that in the port from which I come, West Hartlepool, a club had been spoken of for insuring these vessels only—not as a piece of bravado against those who object to them, but to accommodate those responsible and experienced owners who have found that they are much safer than any other type of cargo boat. This club has now been established; it has been supported very influentially. There it is, now, before the world to show whether it can be sustained; it is a simple "well-deck" club, in which they refuse to insure any other type of vessel.

Vice-Admiral De Horsey: My Lord, I should be glad to be allowed to explain, that in speaking of deck erections I did not allude to such ships as the last gentleman has spoken of. If the deck erections are from side to side, and as strong as the rest of the ship, they do not come properly under the head of deck erections: they are *bonâ fide* part of the ship. I alluded to what we commonly term deck erections, which are merely deck houses.

Mr. B. Martell: My Lord, I am very sorry to trouble the meeting with so many remarks, but these two papers appear to bear so directly upon matters with which my name is associated that you will excuse me, I am sure, for occupying the time of the meeting. I perfectly endorse what Mr. Withy has said. There can be no comparison between the ships which are commonly called well-deck ships now as compared with what they were some years ago; the safety of them has been immensely increased. Mr. Withy has done good service in writing those letters to the *Shipping Gazette* that he has spoken of, where he mentions distinctly various improvements that have been made in these ships during the last ten or twelve years, and shows the great superiority of them now as regards safety. That accounts in a great measure for many of the objections to our present freeboard Tables that are raised by Mr. West, as compared with the previous Tables. I do not know whether he would say (it seems like it, to a certain extent) that having expressed an opinion fourteen or fifteen years ago one is never to learn anything more, never to alter, and never forget anything, because I do not come into that category if he does. The information that we have bearing on loading has been gained after an enormous amount of labour. After the immense trouble that has been taken in preparing these Tables, I am persuaded, as Mr. Glover told the President of the Board of Trade, that ships loaded according to them are not, and cannot be considered, overladen. Shipowners are coming in large numbers now to have load lines fixed for their vessels. As to the freeboard provided, I can assure you that in very many cases we are unable to give the freeboard that shipowners have been in the habit of sailing their ships at, and what they consider would be a perfectly safe line. The freeboards that these Tables give in these cases are greater than shipowners consider necessary, and greater in many instances than that which is in practice adopted by them. At the same time, I may say, that it is not a compulsory load line which Lloyd's Committee wish to force upon shipowners, but a line which, after all the information they have been able to collect, and from all the experience they have had, they believe to be a satisfactory load line, taken as regards actual practical experience over years, of shipmasters and shipowners who have been able to furnish the information from which we have calculated it. Lloyd's Register Committee say, in view of the many difficulties under which shipowners labour now, and the great risks they incur, and the great responsibility they have, and the difficulties there are in the event of the loss of ships, "If you like to come to us we will give you the value of our experience, and information that we have endeavoured to obtain in a *bonâ fide* manner." As far as any kind of science can be brought to bear on the subject, it has been applied, but I may say, that it is just possible to have too much of a good thing, and I think that here in Mr. West's paper we have had a little too much science. When we get such an enormous algebraical equation as this merely to determine what allowance to make for a forecastle, it

almost staggers one to look at it. I find that this large "S," which is put for "structure," should really also represent "surveyor." Because he is the man who has first to determine on the co-efficient according to his own judgment: in other words, you may have every man differing in the basis from which he shall start, and yet, having that uncertain basis to begin with, there is afterwards a most elaborate equation to work out what shall be mathematically correct. I say, if you were to put this equation in other terms, you might, in the first place, "square this surveyor"—(laughter)—and I am sure the surveyors of the society to which I belong would most distinctly protest against that; then go on again to cube him; he is a plus and a minus, and he is all sorts of things. I do not believe for a moment that such elaborate mathematical calculations for a small matter of 43 tons are necessary. It is carrying science a little too far. That is my opinion of that. Even if I examine it with reference to its application, it is not, in my opinion (of course it is natural that I should think so, but I put it before the meeting), the same kind of satisfactory basis as that on which Lloyd's Committee have founded their deductions for erections. They apportion their erections in proportion to a perfectly closed-in deck of the same, and it seems to me that all erections of a substantial character, like those going out to the side of the ship, must bear relation to a similar structure throughout the entire length of a vessel. I saw in a paper some time ago, a letter, in which the writer shows that by the Board of Trade rule you can positively load a ship of this kind with a deck structure lower than you can a ship with an entire awning deck. That cannot be right. Again, by Mr. West's rules, you would get more forecastle allowance for a ship with a great sheer than for a ship without any sheer at all. That cannot be right. Again, a bridge-house is mainly intended to protect the engine-room openings, which would bring it abaft of midships, but by Mr. West's rules its value would be greatest when distributed symmetrically about the midship part, whether it covered the engine openings or not. Then, again, with regard to spar-deck ships, you cannot have a rule, as proposed by Mr. West, applying to spar-deck ships, because there are spar-deck ships and spar-deck ships. We must take into consideration the improvements which have been made in these vessels. Their structural strength is a great deal more than it used to be; they are framed in a different way; their strength has been increased, and it would not be just to say that none of these ships shall load below the main deck. They must be judged of according to their strength and merits; there is no definite rule that will apply to ships of that type altogether.

Mr. C. H. Jordan: My Lord, I understood Mr. Martell to say just now that the load line as proposed by Lloyd's was not to be considered a hard and fast line.

Mr. Martell: I beg your pardon; I said nothing of the kind. I said that Lloyd's Register Committee did not wish to impose upon owners a hard and fast line, but to leave it to them to come to the Committee if they wished to decide upon a line—to let it be a voluntary line.

Mr. Jordan: The remarks of Mr. Martell, and the explanation he has just made, do not alter what I said. I cannot reconcile that statement with a clause in the circular which I have in my hand which has been issued by Lloyd's Committee.

Mr. Martell: What I meant was, not a compulsory load line.

Mr. Jordan: The circular is issued on the 17th August, 1882, and in the 4th clause we find these words: "The maximum load line in salt water determined by the Committee shall, if accepted by the owner, be adopted by him as the load line required by the Merchant Shipping Act of 1876, to be marked on the sides of the vessel." I think that needs some little explanation, because one

can hardly reconcile it with what Mr. Martell has just said. We are quite willing to allow Mr. Martell to alter his opinions from time to time as he gains experience. He claimed to be allowed to do that just now, but I cannot quite reconcile what Mr. Martell has said with this clause. He told us just now that the load line put forth by Lloyd's Committee was to be accepted as a fixed line.

Mr. MARTELL: Yes.

Mr. RUNDELL: What he said was, a satisfactory load line.

Mr. JORDAN: Then he qualified that by saying that it was not to be accepted as a compulsory fixed line.

Mr. MARTELL: I corrected myself by saying it was not to be a compulsory load line.

Mr. JORDAN: But in this circular I think it certainly means that it is to be accepted as a compulsory load line.

Mr. MARTELL: It is optional on the part of the owner to request a line of that sort to be put on his ship. The Committee have no compulsory powers to compel a shipowner to accept that.

Mr. JORDAN: This clause certainly means that it is a compulsory load line that is intended to be put forth.

The PRESIDENT: I do not think it well that we should carry this further. You have expressed your view, and it has been answered.

Mr. JORDAN: I have said, my Lord, all that I wish to say.

Mr. G. RENWICK: As a shipowner, I should like to say a word or two upon this question; not from a scientific point of view, but purely from a shipowner's point of view. I think that gentlemen who endeavour to go into this question from a scientific point of view ought to be thanked by all shipowners; but I for one wish that they would come to some satisfactory conclusion very soon. We have on the one hand the Board of Trade Tables, and we have on the other hand Lloyd's Rules, and now we have another society coming forward and telling us that the conclusion arrived at by at least one of those bodies is erroneous. Now, we have been told very much lately that we ought to consult our captains with regard to fixing a load line, as they are a very important factor in the matter. I have taken great pains lately to consult the captains of every ship with which I am connected as to the sea-going properties of the vessels which they command, and the conclusion I have come to is this, that as far as regards fixing the load line we cannot place much reliance upon their views, for this reason: we put a captain in a ship, and he comes home after a voyage and says, "I never was in a better sea-going ship." We put another man into that same ship, and he goes, say, two or three voyages. After that he comes home and says, "Well, she is one of the most uncomfortable ships I ever was in." Lloyd's fixed a load line for my firm a short time ago on a ship we had at Sunderland, and I was alongside the dock where the ship was lying, and said to the captain, "You have a good ship there, and you have a load line not only placed according to our experience, but it is backed by Lloyd's, so you ought to be comfortable now." "Oh," he replied, "that may be right or wrong; but it ought to be placed there by my experience." "Then," I said, "how are we to get your experience?" He replied, "After we have been at sea a voyage or two." "Well," I said, "what are we to do in the meantime? Who is to place the load line? Are you going to sea without one?" Of course he had nothing to say in reply. We shipowners have to take the

responsibility; we place the load line in a certain place, and the Board of Trade comes and interferes with us. Even after we have placed it according to Lloyd's Tables, which I believe are the best we have before us at the present time, we are told by Mr. Chamberlain that he will not give any guarantee that we may not still be interfered with. Therefore I hope that if Mr. West has some tables to put before us he will get them issued as soon as possible, and let whoever else it is that is undertaking the question arrive as quickly as possible at the basis we ought to go upon. There is one more thing we are told that we ought to consider, and that is, that in loading our ships we ought to consider the cargo that is to be placed in them—that is, according to whether the cargo be heavy or light, or a homogeneous cargo, or whatever it may be. That to a shipowner is almost impossible, because he may have a ship in a foreign port, and may get a telegram that a merchant will give so much for the ship's dead weight capacity. In this case the shipowner really does not know what is going to be put into the ship. Therefore whoever is going to settle this question I think ought to bear in mind that they ought to place a load line upon every ship that will give security for the safety of the ship, no matter whether she is loaded with a light or a heavy cargo, always of course leaving it to the judgment of the captain to take care that she has sufficient stability when she starts on her voyage, which is also a very important point. I do not wish to detain the meeting any further, but really could not resist touching upon this important matter from a shipowner's point of view.

Mr. W. H. White: My Lord, I do not profess to be an authority upon this subject; I say that at the outset; but I think I am right in saying—and I do so with much pleasure—that the Tables which Lloyd's Committe have produced, and which are now published, and therefore open to discussion, have been prepared under other conditions than those apparently represented in these Tables. I mean to say, that although for convenience of reference some dimensions, such as the moulded depth, are taken as ruling the freeboard, as a matter of fact the Tables have been based upon calculations of stability, as well as reserve of buoyancy, made from many representative ships with cargoes put in them. I believe that is so; and perhaps, my Lord, you will allow Mr. Martell to say whether that is not the case. It is one thing, as Mr. John pointed out last night, to have some system of numerals or numbers of reference to guide people in taking out values. There may not be a distinct and direct connection between those marks of reference and the processes by which freeboards, or scantlings, have been fixed. I think that Mr. Renwick made a very important point when he spoke of the different conditions introduced by different cargoes; but would shipowners be content always to have the freeboard fixed with reference to the worst possible cargo which a ship could carry? I simply ask that for information. To take an example, suppose there was a cargo which completely filled the space of the ship, and brought her centre of gravity very high, that should be associated, as Mr. Renwick will admit, with more freeboard and less dead weight than might be safely taken under other conditions of loading with cargoes which might be placed lower down. Now, would a shipowner be content to have taken as the basis of assessment the worst possible species of cargo that could be put in? Because, if not, the information that Mr. Renwick asks for cannot be given, or at least not given in that definite form which he desires. I notice in Mr. West's formula, at page 6, if I correctly understand it, that the co-efficient for the position of the erection may have a curious result, if applied to anything at the end, say a forecastle. A short forecastle at the end of the ship, which might be very valuable, would under that expression, which appears last on the right-hand side, the last multiple, become almost nothing, I believe.

Mr. West: It would become a very small quantity.

Mr. WHITE: Well, that does not seem to me to be just. It may be so as regards transverse stability, but it surely cannot be so with regard to the power of the ship to lift to the sea, and the amount of water which can be kept off the deck. It appears to me that there must be something wrong about that form of expression, especially as dealing with poops, and forecastles, and erections of that kind, which from their position in the ship would not help transverse stability much, and, if you consider them with regard to their buoyancy, would not do very much; but would have very great lifting power, and enable the ship to lift to the sea, and keep the water off the decks. After all, I think it comes to very much the same thing, as Mr. Martell says. If you have effected a long expression of this kind by the judgment of the surveyor, perhaps some shorter process would give an equally satisfactory result. I have looked through Mr. West's specimen calculation, and it seems to me that after all, however algebraic the process may be, there is a very liberal discount on everything that comes above the upper deck proper, and I suppose there must be a disposition in that direction. I cannot sit down without saying that I cordially endorse what Mr. Withy said about the possible value of deck erections, and also the necessity, if they are to have any value, of having few doors in them, or of arranging some means of access from aloft if possible. I am glad to hear from Mr. Withy that the latter method of entrance is becoming the practice. It seems to me that, however intelligent and however capable the captain may be, in cases of emergency it must be better to leave him as little as possible to think about as regards the closing of doors, or openings, so as to get the best possible results out of erections above upper-decks.

Mr. RAYLTON DIXON: My Lord, I wish that Mr. West, in his remarks in this paper upon deck erections, had given us rather more clearly his views as to those, and as to the value that he proposes to attach to different structures on decks, because, for my own part, I think that any deck erection that does not form, as has been said, part of the actual structure of the hull of the ship is really of no value. I think if Mr. West could define rather more clearly how far the discretion of the surveyor shall go in that matter, it would be rather more interesting, and would give us a better idea as to how to value the calculations which in his paper he puts forth. We have rather got into a discussion upon the necessity or advisability of making seaworthy and water-tight the various openings on deck. It seems to me to be a question of surplus buoyancy, because, I take it, if we have an iron house in the centre of the vessel, which may be of the greatest service in protecting the openings to the engine-room and elsewhere, that, as a matter of surplus buoyancy, it is useless if we have near that, or down below the level of that, an opening or a hatchway much thinner or lighter than the deck, and a structure much weaker than that house, which I believe is in many cases very valuable, and which, as I understood Mr. Withy, in his view he would attach considerable importance to as surplus buoyancy. Of course if we close all the openings properly, all over the vessel, the question of surplus buoyancy disappears altogether. You may carry it to the extreme of having Jules Verne's submarine vessel, which is as safe under the sea as on the top. That is, I think, an entirely different question; but to attach a value in the nature of surplus buoyancy to houses on deck, however strong, if close to them you have a light wooden companion, or a light hatchway opening down into the hold of the vessel, does seem to me to be perfectly absurd.

Mr. W. JOHN: My Lord, I quite agree with Mr. Raylton Dixon that there is a great deal more said about surplus buoyancy in connection with this matter of freeboard than it deserves. It is a good sounding phrase, but the moment you depart from a pure flush-decked ship, you lose sight of and control over surplus buoyancy entirely. When you go into the question of spar-decked ships, there is

no reason, on the ground of surplus buoyancy, why you should not load a spar-decked ship quite as deeply as a three-decked ship. The question of strength comes in. The same when you come to the question of a low awning-deck ship, which is a lighter ship, the question of strength rules the matter far more than the question of surplus buoyancy. From the awning-decked ship we come back again to the flush-decked ship. When you come to the deck-houses and poops, and these other matters for which you would look, it becomes a matter of experience with the type of ship, and a matter of judgment and experience, how far these erections are really valuable. You may take types of ships like the ships of which Mr. Withy has built so many very excellent examples; you may follow a practice of loading those ships, and you may find it a safe practice, and if you come to legislate on the load line question, whatever formula you may put it into, you will have to agree practically upon some practice. You may take, as it is sometimes said, a tenth of the buoyancy of the poop, and call that surplus buoyancy, but it is nothing of the kind. It comes to two or three inches on the loading. You may just as well call it by its right name, and say the ship owing to those erections shall go down two or three inches deeper, and drop the nonsense of mixing up surplus buoyancy with it at all. I have pretty much the same objection to this formula of Mr. West's. If I have to guess, I would as soon guess the whole as guess the half and multiply it by two, and I am afraid Mr. West's process is the latter. After all it comes to a matter of close tabulation, and the great value of these Tables, to my mind, is this: that, starting some years ago, when this freeboard question came up very strongly, Mr. Martell took the matter in hand, and tried to tabulate the practice of the country. Since that he has been modifying it, and he has been getting closer, and he may get closer yet, perhaps, to the real safe practice of the country; and I think he is to be congratulated upon the fact that he and the shipowners of the country are getting much more nearly together, and I think that is the direction which will lead to a solution of this very important question.

Mr. H. C. Rothery, Q.C.: As your Lordship has been pleased to call upon me, I will say a few words, not on the subject of the two valuable papers which have been read before you this afternoon, for owing to my engagements elsewhere I was not able to be here when they were read, but on some remarks that have fallen from some of the previous speakers. In the first place, I was very glad to hear what was said by Mr. Withy, in much of which I entirely agree. Mr. Withy has told us, that if upon a flush-deck ship you put a top-gallant forecastle, you add considerably to her seaworthiness, and if you add a poop, you do the same, and thus deck erections become very useful, as they assist ships to rise over the seas which they encounter. You thus, as he says, get to the form of the well-decked ship. But there is one remark which I should like to make in support of what Mr. Withy has said, and that is, that formerly the decks of well-decked ships were often very insufficiently protected, the front of the bridge-house and the doors and openings being frequently very insecure. Now, however, a great number of these well-deck ships have a solid iron bulkhead forward of the bridge-house, which undoubtedly adds very greatly to the strength and security of the vessel. So far, therefore, I agree with Mr. Withy; but there is a danger in these well-deck ships lest the owners should allow too much for the deck erections, and be thereby induced to load them so deeply as to bring the main deck too near to the water. That appears to me to be the only danger, and I hardly think that Mr. Withy has touched upon that point. There is always a fear lest by bringing the main deck too near to the water you render the vessel liable to ship water into the well, thus endangering the lives of the crew who are working her, and by adding considerably to the weight above you disturb the stability of the vessel. Then with respect to the question of the load line. I am unable to discuss Mr. Rundell's paper, for I have not read it, but I quite concur with Mr. Renwick in saying that the sooner the question of the load line is settled, the

better for the shipowners, and, I would add, for the Wreck Commissioners' Court. We want every information we possibly can get, and for my own part I welcome every contribution to the subject, from whatever quarter it comes. It has been my lot for some time past to have to compare the Board of Trade Rules with Lloyd's Rules, and I find it a most remarkable circumstance, that whilst approaching the subject from different sides, they, in the great majority of cases, approximate very nearly. It is but justice to the Board of Trade and to Lloyd's to say this, as it shows the very great care which both those bodies have taken in preparing their Tables. I cannot at all agree with those who would wish to treat in a hostile manner any plan that may be counter to their own. The subject is a new one, and I think we ought to do all we can to welcome any suggestions that may be made, with a view of arriving at some safe and satisfactory conclusion, upon a point which we must all admit is one of extreme difficulty.

Mr. W. W. Rundell: My Lord, I shall be very glad to take this opportunity of saying a few words, but I shall be very brief. Not being a professional man, you must not rely in technical matters upon my authority, but I will just for a moment, by means of a diagram, explain, as I understand it, the mode of assessment of deck erections taken by two different authorities. [Mr. Rundell explained his argument by a drawing on the black-board.] Suppose the vertical line sketched at the middle of the length on the deck of this vessel's profile to represent the value for reserve buoyancy of an erection amidships, and other vertical lines uniformly decreasing in height to nothing at the ends of the vessel to be the values of erections at the intermediate positions, I think it will be seen that we give too small a value relatively to parts which Mr. Withy and others have pronounced to be very important, that is, to the erections near to the bow and to the stern. Now Mr. West gives greater value to the assessment of those erections, by using a parabolic curve instead of a straight line. He gives a little extra value to the end erections, and he gives to erections nearly amidships approximately equal values. A method of assessment that accomplishes this, seems to me to be very valuable, especially if we compare it with a proposed mode of assessment which gives no value to end erections unless they are together equal in length to four-tenths of the full length of the vessel. To refer to another part of the subject, the height of the erection, which reminds one of Mr. Rothery's satirical allusion to the reserve buoyancy in the funnel. Height is a very important element. The enclosed space near the deck is very valuable as reserved buoyancy, but as you go higher the enclosed space becomes less in value. Now if I understand him rightly, Mr. West takes these and other considerations into view in a very satisfactory and easy manner, and although we may not perhaps at first be very ready in making the necessary calculations, we must not be frightened at Mr. West's formulæ, because there is really no difficulty in handling them. The only other remark I wish to make is an expression of pleasure at Lloyd's Register Committee having published these Tables of freeboards. I also congratulate Lloyd's Surveyors on having produced these Tables. The whole of what has been said against them I think amounts to this, that there is a great deal of surplusage. I think Mr. Withy would agree with me in saying that if you have a complete displacement scale and you wish to take any given percentage of it, there is no need to go into co-efficients of fineness for this purpose; that would be unnecessary. There is one rather important matter, and I would like to get an expression of opinion on the point. The circular I hold in my hand (an authoritative document) says that the load line approved of by Lloyd's Register Committee is a maximum load line beyond which the vessel shall not load, because it says that the line represents the disc required by the Merchant Shipping Act. Now Mr. Martell appears to say we must not understand it in that way. This seems to me an important point. He says it is a satisfactory load line, and not a deep load line. If he would kindly

enlighten us on this point, I think he would be giving information which at present is much needed by the shipowners of the kingdom.

Mr. MARTELL: Just a word of explanation; I have been misunderstood in reference to that. I explained to Mr. Jordan afterwards that what I meant was that it was not a compulsory load line. Mr. West appeared to speak of it as if it were a compulsory load line; but I mean that if any shipowner comes and wishes to have a load line assigned by Lloyd's Committee, that is supposed to be the maximum load line beyond which he is not expected to load, and it is for that purpose it is made to conform with the disc that is required to be placed there by Government, so that it becomes illegal if he loads below it.

Vice-Admiral DE HORSEY: Before Mr. West replies, may I ask to be allowed to say a word of explanation? It is merely to say that in advocating that owners and captains should be the judges as to the loading of their vessels, and that there should not be a hard and fast line for all vessels of whatever age, I did not wish to undervalue what is commonly called the Plimsoll Act, nor did I wish on any account to abolish the excellent control which the Board of Trade exercises over unseaworthy ships going to sea.

Mr. H. H. WEST: At this late hour I shall endeavour, my Lord, to be as short as I possibly can. With reference to Mr. Rothery's remark as to the danger arising from the well being filled with water, I think, with all deference to him, that Mr. Withy has already met that point. The smaller you make the well, the less danger there is from that source. I would like to say here, that the erections contemplated in my paper are defined as substantial structural portions of the ship such as Mr. Withy has described. The term "erections" may be a misnomer, but it is well understood. Mr. John says that if he had to guess at all he would just as soon guess the whole as guess the half. I quite agree with him, but I venture to think, notwithstanding his remark, that this is not altogether guess-work. Of all the elements represented in this formula, only one, the element of strength, can have the least fraction of uncertainty about it; and even this is reduced to very narrow dimensions, as it is always referable to the standard of the published rules of the society. And perhaps here I may reply to Mr. Raylton Dixon's question, because I can meet the two things together. Mr. Dixon asks for some indication of the value of the structural factor which the surveyors would adopt for different kinds of erections. I have endeavoured to give some indication of that in the example I have quoted in the Appendix. I think (although I should not like to tie myself absolutely to those figures) that they fairly represent average cases of that type of vessel. Now, Mr. White said that you would get a very small value by this formula for an erection at the extreme end. Undoubtedly you would, but it is a greater value than at first sight appears. Mr. Rundell has made this clear for me by his diagram, from which you will see that our zero value is absolutely at the end, and as we measure to the centre of length of the erection, it is manifest that any erection, however short, will get some value, and as you extend its length from the end, you get a value that is very rapidly rising, and is of considerable moment. I quite admit, as Mr. White says, that there is a very considerable discount taken off the actual capacities of all these erections, and that it must be so, but I contend that it is done consistently and equitably. Mr. Martell said that the value of S for spar decks should be a very different thing, according to their strength. Undoubtedly it should, and that is the very pith of the whole thing; that if you get a different strength you get a different result, and a result varying exactly in the ratio of that strength value that Mr. Martell is so strong upon. If you follow this out with a little thought, I think you will see that when the spar deck becomes as strong as the main body of the ship, the formula puts its

displacement exactly on the same footing as the displacement under the main deck. Then, as to the algebra and arithmetic, and its being a troublesome process, I will only say it is merely a matter of minutes. Perhaps it looks formidable, but with a table of square roots I do not think there need be any difficulty about it in practice. The questions at issue are too important to be put on one side simply because of a little trouble. When you consider that even an inch in the draught of a ship, voyage after voyage, year's end to year's end, amounts to many pounds, it is quite worth while to spend ten minutes in working out allowances by a formula that shall give satisfactory results. In reference to Mr. Renwick's remarks, I think if he reads the paper he will see that I have no intention of doing anything in the way of fixing a load line. There is another remark, made by Mr. Martell, about high sheer that is hardly worth while entering upon now, but I would just say, that as the sheer enters into the displacement below the surface of the deck, you are left with the erections to deal with separately and distinctly. I do not think, my Lord, that I need detain the meeting any longer.

The President: Gentlemen, I am quite sure that Mr. West will not regret the very wise decision that you yourselves came to, that his paper should be considered separately, and I must congratulate you and Mr. West upon the most valuable discussion that has ensued upon it. I cannot help thinking that this discussion is a very considerable step in advance towards the solution of this important and difficult question, and in saying that, I would personally offer my thanks, and I hope you may be induced to join in them, to Mr. Withy for the admirable contribution that he offered to-day to that discussion. I may also be allowed perhaps to say that it is rarely that we hear science so enlivened with wit as we heard a short time ago by our friend Mr. Martell. I was extremely glad to hear another remark from Mr. John, and the more so coming from a man of his experience and weight in this room. He says, and he gives a decided opinion, that we are approaching at any rate to a consensus of opinion among shipowners upon this difficult question of sufficient freeboard. I hope that time may arrive rapidly, because the agitation on this question is neither to the interest of shipowners or shipbuilders, and if we can possibly arrive at something like an agreement I believe the merchant service would largely benefit. I ask your permission to offer our united thanks to Mr. West, and also to those gentlemen who have made such valuable contributions to the subject in the course of this discussion.

A SELF-PROPELLING, SELF-CAREENING FLOATING DOCK.

By G. B. Rennie, Esq., Member of Council.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the chair.]

On the 18th March, 1869, I had the honour of reading a paper before this Institution "On the Iron Floating Dock of Carthagena, its Proportion and Relative Stability;" in which, after describing the capabilities of this dock, I gave the results of certain experiments on the stability of different sectional forms of floating docks, with and without the weight of ships on them. Suggestions were also given as to an improved form of dock for being towed across the Atlantic. The President (then Sir John Pakington) made this remark in winding up the discussion: "I think we are much obliged to Mr. Rennie for the able paper he has given us, combined with his very important practical experience. Although I am afraid it does not bear much upon the question of conveying docks to their ultimate destination, &c." And again, a year later on, he says, "Every naval man who hears me will concur in the opinion that nobody could contribute a more valuable addition to naval contrivances than to invent a really efficient floating dock, and not only a floating dock, but a dock which, when afloat, may go to any part of the world."

I trust, therefore, the above-mentioned remarks will be sufficient reason for the paper before us.

A dock, to do what is required, must not only be capable of being propelled, but must have ready means of cleaning the under-water parts, in order to ensure the speed being maintained by preventing undue resistance, besides all the conveniences of docking ships with safety and expedition. As far as an efficient floating dock for docking ships, this has been solved by the systems adopted both at Carthagena and Bermuda. The cleaning of the under-water parts in both these examples has been successfully accomplished, each in a different manner; but the self-propulsion of a dock combining these qualities has yet to be done.

I propose to accomplish this—

1st. By making the dock of suitable form and strength, so that ships may be easily docked without the use of either gates or caissons (as at Carthagena), and of a form easily propelled at a moderate speed through the water.

2nd. To make the pumping machinery for emptying the dock also serve the purpose for propelling it through the water.

3rd. By giving the dock such a sectional form, that by means of water ballast it may be careened over first one side and then the other up to the keel line.

No. 1 may be best explained by reference to the drawing and model.

No. 2. The means proposed for this is utilising the machinery for pumping out the dock for propelling it, by means of the water discharged in the contrary direction to which the dock is to be propelled.

The example best known of this system of propulsion is that of H.M.S. *Waterwitch*, but which, compared to similar ships, such as the *Viper* and *Vixen*, propelled by twin screws, did not give such a good result of speed for power.

Thus, the *Waterwitch* gave only 9·3 knots, with 760 I.H.P., whereas the *Viper* gave 9·6 knots with 696 I.H.P. This difference seems to me to be fully accounted for by the small propelling area of the water jets, only about $5\frac{1}{3}$ square feet in the *Waterwitch*, whereas the disc area of the two screw propellers was 127 square feet in the *Viper*, or as 1 : 24.

Mr. White, in his "Manual of Naval Architecture," discusses the whole question very clearly, and with his views generally in the matter of water-jet propulsion I quite agree.

In ships of the usual construction the water-jet propellers seem to be difficult of application, in consequence of the large inlets and outlets that would be required for the passage of the water to give a good result as compared with screws or paddles. In the case, however, of a floating dock the case is quite different. Here large pumping power is required for emptying the different compartments, and this can be distributed along the length of the dock on either side, and although the power for emptying the different compartments of the dock would not be required to the extent necessary for propulsion even at a very moderate speed, there would be no disadvantage in having a surplus power for that purpose. The maximum speed that seems to me to be necessary for a floating dock to be propelled to its destination, or from port to port, should not exceed five knots, and probably four knots would be found sufficient. The propelling power for such a speed, with the necessary propelling area, such as would be considered a proper proportion if the paddle or screw were adopted, could easily be distributed in several sets of pumps and engines, with separate suctions and discharges, on either side of the dock; the area, pressure, and velocity of the discharged water being made in proportion to the propelling speed required. In the drawing shown, which is for a dock 350 feet in length, and suitable for ships of 4,000 tons' weight, six sets of engines, pumps, and discharging nozzles are shown on each side, which may be compared in their disposition to the oars or paddles of a boat or canoe.

3rd. The careening of the dock would be effected by utilising the pumping machinery for filling the water-ballast chambers on either side. The engines would then be stopped and all made secure for careening, when the sluice valves on the side to be raised out of water would be opened, the dock would gradually heel over, and assume the position shown in Plate XV. The form of the section, the centre of gravity of weights, and the weight of water ballast have all to be carefully considered and calculated in order that the dock may be careened over, so as to expose the under-water part up to the keel. When one side is done the same operation can be performed for the other side.

In conclusion, it seems that the necessity of dry dock accommodation is increasing all over the world in proportion to the increase of ships both naval and mercantile, and the more general adoption of iron or steel in their construction. There are many places where masonry dry docks are practically impossible to construct, and the time and cost to construct them in other places is a bar to doing so. That patent hauling up slips are only suitable for small vessels under 2,000 tons. That to re-erect abroad or tow a floating dock to its destination, is often a most difficult and expensive matter. That the want of dry dock accommodation would be fulfilled by making it self-propelling and navigable. That the balance of advantages of doing this, is propulsion by means of water jets as described. That such a dock, having all the maritime rights of other ships, would be able to enter and anchor in many ports, and there dock ships without connection with the shore; and although, perhaps, the speed of five knots might prevent it accompanying a fleet, it might meet it at the port of rendezvous and be ready to examine and clean the under-water parts of the fast cruisers and other suitable ships.

DISCUSSION.

Mr. Liggins: My Lord, I think this is a very valuable paper, and a most sensible and practical suggestion. I make that remark because I have had some little experience among the different West Indian Islands of the great inconvenience occasioned, and sometimes the abandonment of ships, in consequence of there being no dock to take them, or any means of getting any disabled ship to the only dock in the West Indies, situated at St. Thomas. If St. Thomas Dock had the power of moving to another island, it might probably be the means of saving for future use a ship of 3,000 or 4,000 tons. The allusion to the *Waterwitch*, in the sense in which reference was made was all very well, but the *Waterwitch* was a comparative failure, because she, for speed purposes, did not succeed in attaining her speed in the same economical way that screw ships do; but still, at the same time, that is no reason this dock should not go at a slower pace, even if at a little extra cost, than she might do if propelled by a screw. Therefore water propulsion is a very nice and ingenious adaptation of the machinery which she has on board for pumping purposes, and that, it

seems to me, would act efficiently at the slow rate of speed at which it was necessary to go through the water. I think, again, that the same application of water-propelling force is most ingeniously applied in the way Mr. Rennie proposes to careen the vessel, and enable him to do those necessary repairs in painting and cleaning the bottom, which are so essential to the preservation of all constructions of iron. I think, therefore, my Lord, that this is particularly, in a mercantile point of view, a very valuable invention, which deserves to be tried on a practical scale.

Vice-Admiral De Horsey: My Lord, I think Mr. Rennie would add very much to the interest of this paper if, in his reply, he would give us a few particulars as to the sizes of the dock, and, within £20,000 or £30,000 or so, of its cost.

Mr. F. K. Barnes: My Lord, I was in hopes that some gentleman more capable than myself would say something in regard to this dock. I do not know whether the dock would be a paying speculation or not, and that, it seems to me, would be a very important point to ascertain; but, supposing that it were desirable to build a dock to effect the object aimed at, I feel quite sure that this plan would meet the requirements in the most economical manner possible. Large pumping power must be provided for cleaning the dock. That power Mr. Rennie has utilised to propel it along, and I think there is, so far as anyone can see, from looking at it, and without going into the details, every reason to believe that he would get a fair speed—a speed sufficient to push her along. He names a very moderate speed of four knots, and I should think that would be quite attainable. I was under the impression that the area of the nozzle of the *Waterwitch* was about 7 feet; perhaps Mr. Rennie could say?

Mr. Rennie: It is $5\frac{1}{3}$ ft.

Mr. Barnes: I was under the impression that it was 7 ft., but I should think from the areas of nozzle that are obtainable—in fact, you could get very large nozzles here if you please, without increase of resistance—the speed would be exceeded. The method of working the dock, which is very similar to the dock at Bermuda, really leaves nothing further to be desired. The dock can be inclined by the large pumping power which pumps water into the upper compartments already provided for it. When inclined the under-water parts can be cleared and overhauled, and I think when it is made perfectly manageable it would be very economical, and the clearing could be done once a year without any difficulty. It is essential, whatever dock you have, that it should admit of being cleaned so that it might be properly taken care of. That is the more necessary in this case if you propose to move from one port to another. Whether or not it would pay, I am not quite clear. Of course the ports at which a dock like this could be used in war time would be limited. I believe it could only be used in our own ports or the ports of our allies, and all other ports would be blocked to it. At the same time I think, under certain circumstances, it would be of immense value for warlike purposes. I have nothing more to say, but that I think the dock and its working are quite practicable, and I should think it is about the most economical mode of effecting the objects aimed at that could be adopted.

Mr. Standfield: My Lord, I should like to make a few remarks on the subject of this dock, which is now before us; more particularly in regard to the careening for the repairs of the dock itself. It has just been stated that it appears to be an efficient way of getting to the bottom of the dock by careening it over in this way (*illustrating*), and that it is like the Bermuda dock. I believe the Bermuda dock has been erected upwards of fifteen years, and that they have careened it only once or twice in that time, because the process is so very dangerous; and I think, if I am any

judge in this matter, that the dock shown on this diagram could not be careened so as to get at the centre as shown on this diagram, and that it would be a very dangerous process as the author of the paper remarks, and would require great care and great knowledge of the laws of stability and displacement before it could be attempted. I think that a proof that it would a very dangerous dock is, that the Bermuda dock has been careened only once, I believe, in that time. Mr. Barnes can tell us whether that is so, and how long it has to go out of use to be prepared for that careening. If it has to go out of use for a long time, several days, or a week, it is a serious matter, and I believe that is the case at Bermuda. I believe it takes a considerable time to prepare for it. My partner, Mr. Latimer Clark, read a paper here some time ago, showing a dock that could be docked in two hours on an even keel, and which would not have to run any of the hazards and dangers of careening, which there are in the case of this and of the Bermuda dock. It is evident if careening were a desirable thing, that vessels themselves might be careened. Now, there are several docks in existence which have never been careened, and their bottoms have never been examined, because there are no ready means of doing it. Again, with regard to propelling, it is quite evident that the engine power necessary to pump out the dock—even to pump it out in half the time that any dock was ever yet pumped out in—would want quadrupling to propel it at anything like five knots an hour. If the same engine power were put on the screw propeller the shafts that lead to the chamber would be made to gear to the screw propeller, and could propel the vessel twice as fast as it could possibly be done with water jets. I do not think that water power is to be thought of for a moment for the purposes of a sea-going dock. There is no doubt that for the purposes of a sea-going dock, twin screws would be much more efficient than water propulsion. The possibility of careening such a dock is very doubtful; the sides should be higher and the dock more of the shape of the Bermuda dock to be careened. I do not consider this dock as nearly as good in shape as the Bermuda dock for careening; this dock is shown as being provided with long projecting ends similar to our self-docking dock, but as these ends have no corresponding portion of sides they must necessarily deprive a dock of this shape of much of its careening power. A dock that requires careening would not be anything like such good value for money as a dock that could lift its sides and bottom in turn out of the water on an even keel whenever necessary for cleansing and painting. It must, moreover, be borne in mind that the sides of this dock could not in any circumstances be brought into action to increase the lifting power of the dock, as is the case with the self-docking floating dock.

Mr. G. B. Rennie: My Lord and gentlemen, I have to thank Mr. Liggins and Mr. Barnes for the kind way in which they have said that the dock, as far as they have seen the thing, is a practical, and economical, and useful apparatus. Admiral de Horsey asked me a question about the size and cost. I give the approximate size in the paper, viz., it was for vessels of 4,000 tons and 350 feet long. It would lift a vessel such as the fast cruisers that are now being made, that is, vessels of 4,000 tons weight and 350 feet long could be easily docked in a dock of the size there mentioned. Mr. Standfield seems to think that careening is a very dangerous matter; in fact, if I understood him rightly, he said also that I said so in my paper. I think, if he reads my paper, he will see that that is not what I say. I said that the calculations of the weights, and the form of the dock, and the centre of gravity had to be carefully made in designing the dock—not careening it, there is little difficulty in doing that with safety. Then he thinks, also, that in consequence of the Bermuda dock having been docked only once or twice, and the operation being very slow, it was a very dangerous and undesirable thing to do in any future docks. How many times the Bermuda dock has been docked I do not know. I believe the operation is very slow—that it is not from the

danger of doing it that it has been done so little, but it is from the fact that the pumping, or the means of filling these chambers, is a very slow process, and it takes a long time to do it. The exact time I do not know, but here, in the case before us, the difficulty is got over by having very large pumping power, which fills the chambers in a very short time. I calculate that the careening ought to be done very easily, so far as the pumping is concerned; it could be pumped up in half an hour. I do not presume that the water could be run out and the dock cleaned in that time, but the dock heeled over on one side, which might be cleaned, and then the same operation would have to go on for the other side. Then with reference to what Mr. Standfield said about screws. He believes the screw propeller would be a far more advisable propeller than the water-jet propeller. He may know more about that than I do, but, as far as my calculations go, I think it would be very difficult to apply screw propellers. I do not know how, in a dock of such a form as proposed, you could put a screw propeller advantageously, and if you have the same propelling area in the form of the water jet, or screw, or any form you like, you have the same means of propelling with the same pressure, and it seems to me you are likely to get the same speed with the same propelling power. There may be some little difference in friction, but I doubt whether at the slow speed there would be. Then there has been mentioned the case of the *Waterwitch*, where it was compared with a similar vessel; the speed compared to the power was less in the *Waterwitch* than in her sister vessel propelled by twin screws, as mentioned in the paper. To anybody who goes into the matter, I think it is clear that the reason of that was the small size of the jets of water which propelled the vessel along. This view is confirmed on referring to the propelling areas. In some of the trials made in the *Waterwitch* the area was reduced, and when the area was reduced for the same power the speed fell off considerably. The converse of that is this: that if you increase the area, taking no other view than that, the chances are that you would have got a higher result rather than a less result in power compared to speed. Mr. Standfield also mentioned the question of lifting the dock with a flat bottom as being preferable to careening. No doubt if you have a flat bottom dock it is, but there are two or three ways of doing it. Mr. Standfield has one plan. The plan we adopted at Carthagena is shown on those photographs before us. That dock was built first of all in a shallow basin. The use of the basin was enlarged afterwards, so that now we have a further use for the basin, that is, the dock will go with the vessel on it into this basin, and a vessel can be hauled in a horizontal line from the dock to the ship, so that the same floating dock for lifting is available for seven ships of 300 feet long each. I do not know that there is anything else to say, except to thank you for the kind manner in which you have received my paper.

The President: It only remains for me to thank Mr. Rennie, on your behalf, for his very valuable paper.

A DESCRIPTION OF A METHOD OF INVESTIGATION OF SCREW-PROPELLER EFFICIENCY.

By R. E. Froude, Esq., Associate.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The main purpose of this paper is to describe and justify a particular method of investigation of the subject of screw-propeller efficiency by means of experiments on models.

The distinguishing characteristic of this method of investigation is that it divides the subject into two principal branches, viz. (1), the efficiency of screws working by themselves in undisturbed water; and (2) the manner in which that efficiency is affected by the screw being brought into conjunction with the hull of a ship, in virtue of the interaction of the screw and hull. We may consider the operations which are the subjects of these two branches of the investigation, as constituent elements of the complete phenomenon of propulsive efficiency, and for distinction we may term the former of the two the element of "screw-efficiency proper," and the latter the element of "hull-efficiency."

It is the latter of these two elements that has been the principal subject of investigation in the model screw experiments which have been in progress for some time past at Torquay; partly because it happens that this branch of investigation is the one which, under present circumstances, is most easily pursued with satisfactory results; partly because this element is, of the two, affected by the greater variety and intricacy of conditions, and is therefore the one most in need of experimental investigation; and partly because the relevancy of the results of an investigation of the "screw-efficiency" element must hinge upon the nature of the modifications introduced by the "hull-efficiency" element. The hull-efficiency element is, therefore, the principal subject of this paper.

Nevertheless, some investigation of the "screw-efficiency" element was, of course, a necessary preliminary to the study of the modifications arising from the actions of the hull, and to this extent the former subject will also have to be treated in this paper. The knowledge which we have as yet acquired onthis subject may be said to be confined to (1) a tolerably accurate and complete determination (in model screws) of those characteristics of performance which are common to all screws of ordinary pattern; (2) a general

appreciation of the character of the differences in design of screw upon which efficiency depends, the amounts of the resulting differences in efficiency being as yet only vaguely determined.

At this point I think it desirable to answer a query which may perhaps suggest itself in reference to the foregoing explanation. Why, it may be asked, should the investigation be thus divided into two branches? Why, for instance, should not a model of any intended screw be tried solely behind a model of the ship for which it is intended, and the propulsive efficiency in the gross be thus ascertained by a single experiment? Such a question is not an unreasonable one, and I am quite prepared to admit that for obtaining such a result, if that were all that were desired, this direct method promises on general principles to be less laborious and more accurate, than the more roundabout method of ascertaining first the efficiency of the screw in still water and afterwards the modifications due to its conjunction with the hull.

Of the reasons for nevertheless preferring the roundabout, or analytical, method, as it may be termed, some are incidental to mechanical difficulties in the experiments, and will appear as we proceed: two important reasons, however, are based on general principles, and may advantageously be stated here.

In the first place, owing to the relatively excessive frictional resistances of both hull and screw blades on a small scale, it is probable that the gross propulsive efficiency of model and screw, which is all that could be ascertained by the direct method above suggested, would differ by an unknown amount from that of the corresponding ship and screw. We could not even conclude with confidence that a comparison between the gross propulsive efficiencies of any two models would truly represent the comparison between those of the corresponding ships. The analytical method of investigation affords the only chance of determining the corrections (analogous to "skin-friction correction" in resistance) which are necessary in order to make the results of model screw experiments truly applicable to full-sized ships.

Secondly, and waiving the foregoing difficulty, unless in the use we propose to make of model experiments, we are to be content with prophesying what will be the performance of a moderate number of individual intended ships with their intended screws; if, in fact we aspire to be in a position to decide beforehand, in any case that may arise, what will be the best form of screw for a given ship, or what will be the best form of ship taking propulsive efficiency into account; the number of combinations of different designs of hull and screw which it would be necessary to try, in order to achieve this result on the direct or "gross" method, would be altogether prohibitive. The analytical method is of advantage here, as it always is where the variety of governing conditions is great, by systematising the conclusions and reducing them to rule, and so enabling the result of any one of an almost infinite number of combinations of conditions, to be inferred from the results of actual trial of only a moderate number of such combinations.

The advantages of an analytical system of experiment over one dealing only with gross results, are in fact analagous to the advantages which a judicious system of book-keeping under headings possesses by comparison with a mere record of total receipts and expenditure; or again, which an algebraical formula for the strength of a given class of structure has over a tabulated statement of strengths of actual structures of different designs and dimensions. In all these cases alike, the statement of the facts in a comparatively complex and elaborate form, is a necessary expedient for giving the required variety of information within a practicable compass.

The first step in the analytical investigation of propulsive efficiency is, as already stated, the division of the total efficiency into the component elements of "screw-efficiency" and "hull-efficiency." It appears convenient to preface the explanation of the constitution of these elements by some description of the experimental apparatus employed.

The screw on which experiment is to be made is mounted (see Fig. 1, Plate XVI.) on the forward end of a shaft 3 feet 6 inches long, the bearings of which are bracketed down from a frame above the water level, and which is driven with mitre gearing at its after-end by a vertical spindle, the top bearing of which is in the above-water frame. This above-water frame is mounted on a delicate parallel motion, constraining it vertically and transversely, but leaving it perfectly free fore-and-aft-wise; and its tendency to fore-and-aft-motion, which consists of the forward thrust of the screw minus the resistance of the mechanism in the water, is measured automatically by means of a spiral spring, recording its extension on a revolving paper cylinder. The whole is mounted on a truck running on the straight and level railway, which extends throughout the length of the experimental tank a foot and a half above the water surface. The vertical spindle which drives the screw is driven by means of cord belts and a system of poly-grooved pulleys, by the truck-wheels, so that by duly speeding these pulleys any desired proportion of rotary speed to linear speed of advance—or, in other words, any desired linear travel per revolution—can be rigorously assigned to the screw. Any desired linear speed can be assigned to the truck by the governor of the engine which drives it. The final cord belt which drives the spindle passes over a system of delicate levers and pulleys, by which the difference in the tension of the two parts of the belt (which is the measure of the turning moment applied to the mechanism) is automatically recorded on the same cylinder as the fore-and-aft force of the frame.

When twin screws are used, each screw has its own frame, the two frames being mounted, at their proper distance apart, on the same parallel motion, the driving belt passing successively over the sheaves on the two vertical spindles, so that the diagram in that case records the sum of the net fore-and-aft forces delivered by, and the sum of the two turning moments applied to, the mechanisms of the two screws.

For eliminating the resistance of the frame-work, &c., to its passage through the water, from the recorded fore-and-aft force, and the friction of the bearings, &c., of the

mechanism from the recorded turning moment, so as to convert these measures into true thrust delivered by, and true turning moment applied to, the screw, various expedients are adopted, which space will not admit of my describing here. I should state, however, that while there is of course some room for slight inaccuracy in both these eliminations, the measure of true thrust is the most trustworthy of the two, and may be considered very accurate. That of the true turning moment appears liable to errors of some importance—sometimes of excess and sometimes of defect;—the error, however, does not often vary suddenly or rapidly, so that comparative turning-moment results of experiments included within a short interval of time, may be trusted with more confidence than their absolute results.

In the experiments, the truck carrying the apparatus above described is joined to the somewhat similar truck running on the same railway, which is used for experimenting on the resistances of models, the model being for this purpose attached beneath it. When the two trucks are joined the model may either be attached in its place or omitted, and the screw experiments accordingly made either behind model or in undisturbed water, as desired; or, again, either the screw may be removed from the shaft, or the trucks disconnected, so that the model also can be tried either alone or with screw working behind.

The apparatus that carries the screw measures the thrust, speed, turning moment, and revolutions per minute, consequently the experiments on screw working without model in front determine the element of "screw-efficiency" proper; that is to say, they show the power expended in performing a given amount of useful work by maintaining a given thrust at given speed. The experiments on resistance of model alone, measure the useful work to be done. The comparison of these records with those yielded by corresponding experiments with model and screw in combination, show the modifications introduced into the results by bringing the model and screw into conjunction, and these modifications constitute the "hull efficiency."

Now, these modifications are two in number, viz., the effect of presence of screw upon resistance of model, and the effect of presence of model upon efficiency of screw. The former effect consists in what has been termed the "augmentation" of the resistance of the model by the action of the screw, an effect which is necessarily a loss of efficiency, the amount of this loss being directly measured by the comparison of the resistances of the model with and without screw working behind.

The second mentioned effect consists in the fact that, behind the model, the screw is working in water in a certain state of motion, and that, in consequence of this motion, to maintain a given thrust at given speed behind the model, consumes a different amount of driving power (in all ordinary cases less) than in undisturbed water. This difference in consumption of power is, of course, indicated by the comparison of the recorded performances of the screw with and without model; but this comparison, when pursued in detail, indicates not the amount merely, but also the cause of the difference in consumption

of power. This cause is the forward motion of the wake water in which the screw is working, and our experiments prove that, complex and varied as are actually the motions of this wake water, the nett effect of this state of motion upon the screw, is practically identical with that which would be produced by a mere uniform forward current, the forward speed of which, and the saving in driving power due to which, may be measured in a very simple and trustworthy manner by the method of experiment described.

To show how this is the case, we must study the matter in greater detail, commencing with the performance of screws in undisturbed water.

First, then, let us suppose a set of experiments with the apparatus already described, to be made with any given screw without model in front, at one linear speed but at various rotary speeds or revolutions per minute, ranging, let us suppose, from the revolutions which would about give no thrust (and at which, therefore, the screw would nearly turn of itself), to about twice that number of revolutions; in other words, from no slip, to about 50 per cent. slip. The apparatus, it has been stated, measures revolutions per minute, thrust, and turning moment, and the results of the experiments may therefore be represented by diagram in the manner shown in Fig. 2 (Plate XVII.) where the thrust and turning moment are plotted as ordinates to an abscissa scale of revolutions per minute.

We thus obtain curves of thrust and turning moment for a given screw at given speed and varying revolutions. Such curves possess the following characteristics, which are common to screws of almost every design. The thrust and turning force curves both of course ascend with increasing revolutions. They are slightly concave upwards; but, nevertheless, do not become even approximately tangential to the base line, which, if they are produced far enough, they intersect at an angle: such intersection being necessarily at higher revolutions in the thrust curve than in the turning moment curve.

Now for the measurement of efficiency. At any given number of revolutions per minute, the energy in foot-pounds consumed per revolution in driving the screw, is equal to the turning moment in foot-pounds of moment multiplied by 2π. The energy delivered per revolution by the thrust of the screw, is the thrust in pounds multiplied by the linear travel or advance, per revolution, in feet. The efficiency, therefore, being ratio of energy delivered to energy consumed, is the former divided by the latter, namely:

$$= \frac{\text{thrust} \times \text{travel per revolution}}{\text{turning moment} \times 2\pi}$$

or, which is a more convenient form—

$$= \frac{\text{thrust}}{\text{turning moment} \times \dfrac{2\pi}{\text{travel per revolution}}}$$

If, then, we multiply all the ordinates of the turning-moment curve by the factor $\frac{2\pi}{\text{travel per revolution}}$, we get a new curve, such that the ratio between its ordinate and the corresponding thrust curve ordinate at any point in the diagram indicates the efficiency at that point. This curve is also shown in Fig. 2.

It will be seen that the effect of multiplying the turning-moment by the factor $\frac{2\pi}{\text{travel per revolution}}$, is simply to convert it from absolute turning *moment*, or turning force measured at a unit radius of one foot, into turning force measured at a radius whose circumference is equal to the linear travel per revolution. The new curve, therefore, represents the turning force measured in this manner; for convenience, we may term it the curve of "turning force," as opposed to "turning moment."

The "thrust," then, divided by the "turning force" (or turning moment $\times \frac{2\pi}{\text{travel per revolution}}$), measures the efficiency; the ratio of the ordinates of the thrust and turning force curves may accordingly be measured at successive points in the diagram, and an efficiency curve thereby constructed. Again, see Fig. 2.

The prominent characteristics of this efficiency curve are common to almost all screws. Its ordinate is necessarily zero at the point where the thrust is zero, and, like the thrust curve, it intersects the base line at an angle. It rises steeply and uniformly at first as the rotary speed increases, but rounds off and becomes gradually level at the point where maximum efficiency is obtained, whence it descends again gradually. The point of maximum efficiency is consequently somewhat indefinite, and it results that a given screw, advancing through water at given speed, is almost equally efficient within a large range of thrust value.

The diagram, or set of curves just described, viz., thrust and turning force curves, and their derivative the efficiency curve, exhibits completely the performance of a given screw at given linear speed of advance through water. Similar diagrams may of course be constructed for other linear speeds, and this might presumably be done by making several sets of experiments at various speeds, if necessary. There is, however, a very simple theoretical law for expressing the relation between such curves for different speeds, which experiment proves to be very fairly correct, even for the greatest differences of speed that we ever have to deal with, and which, for minor differences, may be considered as absolutely accurate. This law we will now consider.

Suppose, then, that we commence with a diagram for a given speed = V, and that in this diagram, at revolutions per minute = R, we have thrust = T, and turning force = F. Let the speed be now changed to V_1. If the revolutions per minute remain = R as before, the travel per revolution will be increased, the angles at which the various parts of the screw blades cut the water will be changed, and the conditions of operation will be thereby metamorphosed in a manner which will defy accurate calculation. If, on the other hand, the revolutions per minute are changed in the same proportion as the speed, and become R_1 ($= R\frac{V_1}{V}$), the travel per revolution remains unchanged, so does the slip-ratio, and so also do the angles at which all parts of the blades cut the water. The stream-line motions involved will consequently be unchanged in arrangement, the speeds at all points in the stream-line systems being simply changed in the same ratio

as the linear speed of advance. The directions and relative proportions of all forces acting on all parts of the screw blades, whether due to friction or pressure, will therefore remain unchanged, and their magnitude will be simply proportional to the square of the linear speed of advance. The items or component forces being alike changed in this ratio, the totals or resultant forces of thrust and turning moment, will be changed in the same ratio, and so will also the "turning force," since this is turning moment $\times \frac{2\pi}{\text{travel per revolution}}$, and the travel per revolution remains the same.

In changing speed V into V_1, then, the original revolutions are multiplied by $\frac{V_1}{V}$, and the original thrust and turning force are alike multiplied by $(\frac{V_1}{V})^2$. The efficiency, therefore, which is thrust divided by turning force, remains unchanged.

The law of the relation between the performances of a given screw at two different speeds, is, in fact, in one way analogous to the "law of comparison," which expresses the relation between the resistances of ships and models, or similar ships of different sizes. Just as the latter assigns a ratio between the respective resistances, at certain respective "corresponding" speeds, and only at those speeds, so the former assigns a ratio between the respective thrusts and respective turning forces of a given screw at different speeds, at certain respective "corresponding" revolutions per minute, and only at those revolutions. These "corresponding" revolutions are proportional to the speed, and give travel per revolution constant and slip ratio constant; and at these "corresponding" revolutions, the thrusts and turning moments are proportional to the square of the speed, and the efficiency is constant.

To illustrate as completely as possible the relations which these propositions establish between the diagrams severally expressing the performance of a given screw at different speeds, I show in Fig. 3 (Plate XVIII.) a series of thrust, turning force, and efficiency curves for a given screw at four different speeds. A_1 E_1, A_2 E_2, &c., are the several thrust curves, a_1 e_1, a_2 e_2, &c., the corresponding turning force curves, and aa_1 ee_1, aa_2 ee_2, &c., the corresponding efficiency curves. It will be seen that the four curves of each kind must be reproductions of one another on different scales, the zero of ordinate and abscissæ being common to all, the abscissæ scales of all being proportional to the speed, the ordinate scales of the thrust and turning force curves being proportional to the square of the speed, and that of the efficiency curves being constant.

It also follows that if any points B_1, C_1, D_1, E_1 be taken in the thrust curve A_1 E_1, and corresponding points b_1, c_1, d_1, e_1 in the corresponding turning force curve, at certain revolutions per minute; and if points at the "corresponding" revolutions to these—that is to say, at revolutions bearing the same ratio to the speed and giving the same slip ratio, —be taken in the thrust and turning force curves of other speeds, as shown at B_2, C_2, D_2, E_2, and b_2, c_2, d_2, e_2, &c., &c.; and if the successive points of equal slip ratio B_1, B_2, B_3, B_4, b_1, b_2, b_3, b_4, &c., &c., be joined by curves, these curves will be parabolas, originating at the zero of revolutions. Also, if the corresponding points bb_1, cc_1, dd_1, ee_1, bb_2, cc_2, dd_2, ee_2,

&c., be taken in the efficiency curves, the successive points bb_1, bb_2, bb_3, bb_4, &c., will fall in a straight and level line.

To still further complete the conception of the performance of a given screw at all speeds, which this diagram affords, we can imagine any additional number of thrust curves introduced so as to fill the whole diagram with a series of thrust curves corresponding to a series of minute gradations of speed. Any number of parabolas, similar to B_1, B_2, B_3, B_4, may be likewise added, and the successive intersections of any one of these with the several thrust curves, will be points of corresponding revolutions per minute, of identical travel per revolution, identical slip ratio, and identical efficiency. Each of these parabolas is, then, characterised by its appropriate efficiency value, which value may be determined by noting the ordinate of the efficiency curve for any one speed, at the point at which that parabola cuts the thrust curve for that speed; so that since turning force is thrust divided by efficiency, a single efficiency curve, in addition to the thrust curves and parabolas, is sufficient, without any turning force curves, to enable the diagram to show the whole performance of the screw at any speed.

I should here point out that the same train of reasoning also establishes a theoretical law for expressing the relation between the performances of similar screws of different absolute size; namely, that at the "corresponding" revolutions, *i.e.* the revolutions of identical slip-ratio, the efficiency is constant, the thrusts and turning forces being, for given linear speed, proportional to the squares of the dimensions. Here, however, we are immediately concerned only with the law for the same screw at varying speed.

Having thus prepared the way by a full consideration of the characteristics of the performance of a screw in undisturbed water, we are in a position to determine what its performance will be when working in the wake water astern of a model, supposing that wake to consist simply of a forward current having the same speed in all parts of it which come within the operation of the screw.

Supposing the speed of the model through the surrounding water to be V, and its speed through the wake water in which the screw works to be V_1 (so that the forward speed of the wake water is $V - V_1$), it is clear that the screw will be circumstanced precisely as when working in undisturbed water at speed V_1. If working at the same revolutions, the thrust will be the same, and so also will the turning *moment*. The linear travel or advance per revolution will, however, be greater in the case of the screw behind the model in proportion as V is greater than V_1, and the "turning *force*" (as measured for efficiency) will therefore be less in the same ratio. The thrust being the same, the efficiency of the screw working behind model will accordingly be greater than in still water, in proportion as V is greater than V_1, and if we call the efficiency in the still water $= E$, the efficiency behind the model will be $E \times \frac{V}{V_1}$.

Or another way of stating the same result is to say, that the screw being identically

circumstanced relatively to the water it works in, the driving power consumed by it in the two cases is the same; but that the work realised is greater behind the model in proportion as V is greater than V_1.

The measure of the gain in efficiency due to wake is, then, the proportion between V and V_1, and at this speed V_1 in undisturbed water, the thrust of the screw is the same for the same revolutions as behind model at speed V. Now from what we have seen above (see p. 237, and Fig. 3) concerning the performance of screws in still water, it is obvious that a given thrust with given revolutions in still water (a given single "spot," in fact, on the diagram shown in Fig. 3), is in a given screw, consistent with but one linear speed, and no other. Consequently, just as we may say that, given speed of model V, and speed of wake $V - V_1$, the thrust for given revolutions behind model will be that appropriate to speed of screw V_1 in still water; so we may say, conversely, that given speed of model V, and given the fact that the thrust and revolutions of screw working behind model are those appropriate to speed of screw V_1 in still water, $V - V_1$ must be the speed of the wake.

If, therefore, we find that with model speed V we get a certain thrust for certain revolutions with screw working behind it, and that we get the same thrust for the same revolutions in still water at speed V_1, we know that the speed of wake must be $V - V_1$, and that the efficiency of the screw when working behind the model must be its efficiency in the still water experiment multiplied by $\frac{V}{V_1}$. The criterion of the speed of the wake, therefore, and of the consequent gain in efficiency, is the comparison of the thrust curves given by the experiments behind model and in still water. And it should be noticed that in order to determine this speed V_1, at which the required relation of thrust and revolutions subsists, it is not in practice necessary to actually try the screw in still water at exactly that speed (which might involve a lengthy process of trial and error), because the propositions already detailed (p. 237), concerning the relation of the performances of a given screw at different speeds, enable the thrust curve for any desired speed in still water to be inferred from experiments at a somewhat different speed.

These propositions concerning the effect of the wake of the model upon the performance of the screw, would be strictly true if the actual wake were, as here assumed, a uniform homogeneous forward moving current. They will still be actually true in so far as the motions of the actual wake are in effect equivalent to this. Now, our experiments go to prove, as was stated in p. 235, that, complex as are the actual motions of the wake water, their nett effect upon the operations of the screw does not in fact materially differ from that of a homogeneous forward current. Let us now consider to what this statement amounts.

What are the facts yielded by the experiments behind model and in still water, taken together? We have (1) the thrust and revolutions of the screw working at speed V behind model; (2) the thrust and revolutions, each identical with the foregoing, at speed V_1 in still water; (3) the turning moment at the same speed in still water, which we generally

state in the form of turning force as measured for efficiency. We then infer, on the assumption of a uniform wake, that the turning moment behind the model will be the same as in still water, and that the turning force will therefore be less in the ratio in which V_1 is less than V. But we also have, recorded by the experiment, (4) the actual turning moment behind model, which in the same manner we convert into turning force; and the degree of agreement between this actual turning moment or turning force (it does not matter in which form the comparison is made) with that calculated from the experiments in still water, is a criterion of the sufficiency of the assumption that the disturbance of water which we term the "wake" amounts in effect to a mere uniform forward current. Now this comparison has been made in a vast number of experiments, including a great variety of conditions differing in character and amount of wake, and the agreement, though not in all cases quite exact, is so nearly exact as almost to be covered by the limits of error of the observations. The general direction of this difference, such as it is, is the opposite of what one would be at first inclined to expect, since it shows that the actual turbulent wake left by the model increases the efficiency of the screw more than would the hypothetical uniform wake, the efficiency given by the turning force recorded in the experiment behind the model being higher than that calculated from the experiments in still water by an amount varying from *nil* to about 2 per cent., in all the various conditions which have been subjected to experiment. I believe the variations, at any rate, in the amount of this difference, to be due chiefly to inaccuracies in the record.

I am therefore certainly of opinion that although at some future day it may prove instructive to study the effect of the turbulence of wake upon the efficiency of screws, in addition to the effect of its general forward motion, it is certainly desirable to leave the former element out of account until other vastly more important operations have been investigated.

It accordingly amounts to this, that in order, in the case of any individual model and screw, to determine the "hull efficiency," or in other words the propulsive efficiency in so far as it is affected by the characteristics of the hull, and is not determinable by experiments in still water; the only records we have to make use of are (1) the resistances of model with and without screw, which give the loss by "augmentation" of resistance, and (2) the thrusts of screw with and without model, which give the gain by "wake." Records of turning force behind model are not required at all, and although in most of the experiments which I have made such records have been taken for the purpose of testing the validity of the hypothesis on which the measure of the "wake" gain is based, they serve no other purpose. The resistance and thrust records above mentioned evaluate the two elements of "augmentation" and "wake," which together constitute the "hull efficiency," and whatever further information is required to determine the total propulsive efficiency can be obtained from experiments with screws alone.

The experimental investigation of the whole subject of propulsive efficiency accordingly

divides itself into two sections: (1) Experiments on screw thrust and model resistance, to determine "hull efficiency;" and (2) Experiments on thrust and turning force of screws in still water, to determine "screw efficiency." I will presently show how this system of inquiry is put in practice, and what are the conclusions of practical importance it may be made to yield; but it is first necessary to describe rather more fully the form in which the resistance and thrust records which determine the hull efficiency present themselves.

Fig. 4 (Plate XIX.) is a typical diagram showing the results of a single set of such experiments, viz., the complete set necessary for ascertaining the "augmentation" and "wake" values for a single model with its screw, at one speed which we will call V. The set comprises the three essential kinds of experiment, viz. (1) Experiments on model without screw; (2) Experiments on model and screw together; (3) Experiments on screw alone.

The last two kinds of experiments, namely, those made with screw, each comprise several successive experiments at different numbers of revolutions per minute. The recorded forces in these are plotted as ordinates to a base of revolutions, and the "spots" so given are joined by curves. We thus get three curves: (1) the curve of thrust of screw behind model; (2) the curve of corresponding (augmented) resistance of model; (3) the curve of thrust of screw in undisturbed water. The ordinate representing the resistance of the model without screw may be indicated by a level line drawn across the diagram.

The experiment being made for the purpose of obtaining the "augmentation" and "wake" for a particular speed of model, it is essential that the experiments involving the model should be made at that speed. But there is no necessity that the experiments on the screw without the model should be made at any exact speed. For convenience it is desirable that these experiments should be made as nearly as possible at the speed V_1, viz., the speed of model minus the speed of wake; but all that is really necessary is that the speed should be sufficiently nearly equal to V_1 to enable the thrust curve for the speed V_1 to be inferred by means of the theoretical law of the relation of the thrust curves of a given screw at different speeds (see above p. 237).

Now, when experiments such as we are here considering are being made, it is generally the case that the curve of resistance of model without screw at varying speed has already been ascertained; not unfrequently, also, the thrust curve of the screw in still water, at a speed sufficiently near to V_1, has already been ascertained for other purposes. It may be thought that in such cases the special experiments on model without screw and screw without model, to compare with those with model and screw together, might be dispensed with. But it must be remembered that in the experiments we are now considering, we are concerned with not the absolute values of the resistance of model and thrust of screw, so much as with the differences between those values when model and screw are separate and when in combination; therefore, in view of the slight gradual or occasional changes in resistance of model and in thrust curve of screw which are found to arise (mainly, as I imagine, from variations in surface-resisting quality), it is desirable to make all the

experiments concerned in the measurement of the "augmentation" and "wake" in any given case, within a short time of each other. It is, therefore, generally necessary to take special experiments on model without screw, and screw without model, to compare with each set on model and screw in combination.

Let us now examine the character of the curves which exhibit the results of these experiments. We will first deal with the results of model and screw in combination, which consist of a thrust curve of screw, and curve of corresponding resistance of model. The point to be noticed here is that as the thrust increases, so also does the augmented resistance, thus showing that the augmentation is not due to the mere presence of the screw in its position, but to the amount of thrust produced, the suction exerted in front of it, which causes the augmentation, being the essential counterpart of the sternward ejectment of water, which produces the thrust. The augmentation is therefore a function of the thrust, and we ought to regard it as essentially a percentage drawback or discount from the generated thrust, whereby the thrust available for overcoming the natural hull resistance, is less than the thrust actually generated. It is therefore better to term it "thrust deduction" than "resistance augmentation," and the term "thrust deduction" has for this reason superseded "augmentation" in the notation used in the experimental establishment at Torquay. I shall therefore use the term "thrust deduction" in the remainder of this paper. The "thrust deduction," then, stated as a factor of the total efficiency, is the discounted thrust (or thrust *minus* augmentation of resistance), divided by the total thrust; or, stated as we more commonly state it, as a percentage, it is the percentage of the total thrust whereby the discounted thrust is less than the total, or in other words, the percentage whereby the value of the "thrust-deduction" factor is less than unity.

The absolute amount of the augmentation or "thrust deduction" is not, however, exactly proportional to the thrust; it may be more truly described as consisting of two terms, one proportional to the thrust, and the other a constant, this constant being the value of a certain small amount of "thrust deduction" which is found to exist even when the thrust is zero, as indicated by the dotted lines in Fig. 4. The explanation of this phenomenon lies apparently in the fact that the forward speed of the wake is not uniform throughout the sectional area operated on by the screw, whence it arises that when the slip of the screw in reference to the mean speed of the wake is zero, this mean slip is the resultant of a positive slip in some parts and a negative slip in others, so that in some parts the screw is creating thrust and making suction in front of it, and in others is conversely backing water and making pressure in front of it. If these regions of diminished and increased pressure in front of the screw were equally close to the surface of the hull, we should expect the consequent resisting and assisting forces on the hull to balance one another, and so produce no nett effect. We know, however, that it is the part of the wake nearest the surface of the hull that must have the greatest forward speed, consequently the regions of positive slip, and diminished pressure in front of screw, must be

nearer the surface of the hull than the regions of negative slip and increased pressure, so that the former must operate more effectively to increase resistance than the latter to diminish it.

We now come to the consideration of the two thrust curves of the screw with and without model. These will more or less closely coincide, according to how closely the actual speed of the experiments without model, and which we will call U, approximates to the speed V_1, or speed of model *minus* speed of wake. If the two curves coincide, then $U = V_1$, and the wake factor of efficiency $= \frac{V}{U}$. If they do not so coincide, we have to determine the value of V_1, by calculation from the known value of U, and this is done by the following construction, which is based on the propositions concerning the relation between the thrust curves of a given screw at different speeds (see above, p. 237).

In the thrust curve behind model (again see Fig. 4), take a point A, corresponding to revolutions per minute = R, say. Through this point describe a parabola, originating at the zero of revolutions (*i.e.*, such that the ordinates vary as the square of the revolutions), continuing it so as to cut the thrust curve of screw without model at the point B corresponding to revolutions = R_1, say.

Then $V_1 = U\frac{R}{R_1}$; the wake factor, consequently $= \frac{VR_1}{UR}$.

This is the "wake" value for the revolutions corresponding to the point A; and in the same manner the wake value for revolutions corresponding to any number of other points C, E, G, &c., may be found by describing the parabolas CD, EF, GH, &c. Now the wake is a creation of the model, not of the screw; there is therefore no *primâ facie* reason why the wake should be different for different revolutions of screw, and we should accordingly expect to find that the thrust curves with and without model coincide throughout if at all, and if not at all, that the revolutions corresponding to the points D, F, H, &c., bear severally the same proportions to the revolutions for points C, E, G, &c., as the revolutions for B do to the revolutions for A. As a fact, however, this is not always the case; the thrust curve behind model, in the largest class of conditions we have subjected to trial, being somewhat less steep than that which would give the above result, thus indicating that the wake diminishes slightly with increase of thrust. In another (but a smaller) class of conditions, the difference is the other way, the wake increasing with increase of thrust.

The cause of this result, like that of the variation of thrust deduction with variation of thrust, already referred to, appears to lie in the non-homogeneity of the wake. This non-homogeneity results in the fact that the mean forward speed of the portion of the wake comprised within the disc area of the screw often differs from (is generally greater though sometimes less than) the mean forward speed of the zones surrounding that disc. Now when the screw is working with slip, and exerting thrust, there can be no question that the supply of water to it demands a convergence of streams towards it from without its

own disc area, and that the degree of this convergence, and extent of the surrounding water effected by it, must increase with increasing slip or thrust of screw. Consequently, it is evident that the extent of the wake dealt with by the screw will be dependent on the thrust, and consequently the value of its mean effective speed will increase or diminish as thrust increases, according as the speed of the surrounding zones is greater or less than that comprised within the disc area of the screw.

Thus although, as was pointed out above (p. 240), we may advantageously assume, that the non-uniformity of the wake in no way invalidates the measure of the effect of the wake upon the efficiency, which the experiments afford on the assumption of a uniform wake; nevertheless, we see that this non-uniformity does appreciably affect the general result, by causing the values of the "thrust-deduction" and "wake factors," so measured, to vary with variation of thrust in an unexpected manner.

These values, as obtained at different points in the diagram, may be plotted to the revolution values to which they belong, as shown in the diagram, thus yielding curve of "wake" and "thrust-deduction."

The diagram, Fig. 4, shows also two other curves of force record, viz., the curves of "turning-force" of screw with and without model. In describing this diagram I have hitherto avoided referring to these, because, as already explained, they play no part in the measurement of hull-efficiency. I have shown them in the diagram, however, because thus far these records have almost always been obtained in our experiments, for the purpose, as above stated, of testing the validity of the assumption that the effect of the actual wake upon the efficiency of the screw does not materially differ from that of the hypothetical uniform wake. An explanation of the way in which this test is applied will perhaps be useful, as an additional illustration of the meaning of this assumption.

Let us first suppose the speed U of the experiments without model to be exactly equal to V_1, so that the two thrust curves exactly coincide. Then if the wake were uniform, its effect would be to increase the efficiency given by the recorded performance of the screw in still water, by multiplying it by the wake factor as found from the thrust curves, or in other words, to diminish the turning force by dividing it by that factor. Hence, in so far as our assumption is correct, we should in such a case find the ratio of the ordinates of the turning force curves, as actually recorded with and without model, to be exactly equal to the wake factor. In fact, the thrusts being equal, the turning force behind model will be less than that without model, in exactly the reciprocal of the ratio in which the efficiency behind model is increased by the wake.

If, however, as in this diagram, the two thrust curves do not coincide—*i.e.*, if speed U does not equal speed V—the first step is to infer from the thrust and turning-force curves without model at speed U, the turning forces corresponding, in still water at speed V_1, to the thrusts recorded behind model, which is done thus. At the revolutions for the point

A in the thrust curve behind model, we know the still-water efficiency for speed V_1 to be equal to that at the point B in the thrust curve without model at speed U, these being points of "corresponding" revolutions or identical slip ratio, and in the same way at the points C, E, G, &c., the efficiency will be the same as at D, F, H, &c. Consequently the still-water turning forces will severally bear the same ratio to the thrusts A, C, E, G, &c., as the ordinates *b*, *d*, *f*, *h*, of the turning force curve without model do to the thrusts B, D, F, H, &c. We thus obtain the ordinates *a*, *c*, *e*, *g*, &c., forming a new curve *a c e g* of still-water turning force to correspond to the thrust curve behind model, and the ratio of the ordinates of this curve to those of the actual curve of turning force behind model should, as just now pointed out, be equal to the value of the wake factor as found from the thrust curves. In this diagram, which shows the actual result of one of our sets of experiments, the agreement happens to be exact.

As already stated (p. 240), the agreement is not always so exact, the actual turning force behind being less than that inferred from the experiment without model, by an amount varying from *nil* to 2 per cent. of the total. I am certainly at present inclined to attribute at any rate the fluctuations in this difference, to errors in elimination of the friction of the mechanism driving the screw. This suggests another point that should be noticed before we leave the consideration of these curves.

Assuming the object of an individual set of experiments such as we have been considering, to be merely the determination of the total propulsive efficiency in the individual combination of model and screw with which the experiments were made (and we shall see presently that this is, at present at least, never the sole object, and rarely the most important object, of such a set of experiments), we see that the results provide two alternative methods of measuring this total efficiency, which two methods, in the case shown in Fig. 4, give the same result; as indeed, if the records are accurate, they are bound to do in so far as the assumption in reference to the wake effect is a correct one. One of these methods bases the estimate of total efficiency on the efficiency of screw behind model as actually recorded; the other on that efficiency as calculated by multiplying the recorded efficiency of screw without model, by the "wake" factor found from the thrust curves.

Now, it was stated in p. 234, that any individual set of experiments taken nearly at the same time are occasionally liable to a general error of tangible amount in the measure of turning force, owing to the difficulty of accurately eliminating the friction of the mechanism. To this kind of error, either of the two above-mentioned methods of estimating the total efficiency in any individual case, may be equally liable; and of the two, the first method, which bases the estimate on the efficiency actually recorded behind model, would appear to be preferable, as excluding whatever small error may be involved in the assumption respecting the effect of "wake."

But it is generally the case that the set of experiments with screw without model is only one of a large number of similar sets, with the same screw (and sometimes with

others very similar to it) taken on different occasions to compare with experiments either behind other models or behind the same model in different positions or at different speeds. And although these various still-water experiments have generally been made at various speeds, yet by aid of the propositions described in p. 237, they can be reduced to a common speed and compared, and thus an average curve of efficiency for the screw in still water can be obtained, which is much better authenticated (in view of the occasional errors in the measure of turning force) than a curve of efficiency given by any individual set of experiments. By using the still-water efficiency given by this average curve, and multiplying by the wake and thrust-deduction factors, a more trustworthy estimate of the total efficiency can be made than by merely multiplying the efficiency given by the actual experiment behind model by the thrust-deduction factor.

Hence, what I have above termed the second method, thus modified, affords a better measure even of the total efficiency in any individual case than the more direct method of using the actual recorded efficiency behind model as the basis of calculation. I therefore consider that we are fully justified in regarding the measures of turning force taken behind models in various cases, as useful only as a test of the general correctness of the assumption respecting the effect of wake upon efficiency; and for this purpose they are fairly well adapted, since the two records, with and without model, the comparison between which affords the test in question, being taken almost at the same time, it is probable that whatever error may arise in the turning force measure from the inaccurate elimination of the mechanical frictions, will be common to both.

We will now consider what are the results of practical importance that may be obtained from the kind of experiments that I have been describing.

We have just seen that the element of screw efficiency should be obtained, not from the experiments behind model, nor solely from those without model taken in connection with them; but from an average curve of efficiency otherwise obtained. In considering the experiments in connection with models, we are, therefore, concerned solely with the "hull efficiency," viz., with the "wake" and "thrust-deduction" factors of efficiency.

We have seen that these latter generally differ somewhat at different points in the diagram, as in Fig. 4. Now, the diagram at the point where the curve of thrust behind model intersects the curve of augmented resistance, shows what would be the state of affairs if the model was propelling itself. At the higher values of revolutions the condition corresponds to that of the model propelling itself against a head wind, or if towing another model; or at lower revolutions, to that of the model if under sail. Of these conditions, the first mentioned (*i.e*, when the thrust = augmented resistance, which, for brevity, we will term the condition of T = AR) appears at first sight the only important one. But in considering to what state of affairs in the ship this condition in the model corresponds, we have to remember, that before applying the law of comparison of

resistances of ships and models to the calculation of a ship's resistance from that of her model, a correction has to be taken off the model's resistance, on account of the excessive effect of skin friction on a small scale. The actual condition of T = AR in the model, therefore, corresponds to the condition in the ship with resistance enhanced by foul skin. For comparison with the ship in her natural state, to take the model condition, not of T = AR, but of thrust (as reduced by thrust-deduction) equal to resistance as corrected for skin friction, would be a step in the right direction, but would not be quite right either, because the amount of the wake depends largely upon the amount of the skin friction, and so, though we should then have the correct thrust to compare with that of the ship, we should have the excessive wake due to the excessive skin friction of the model.

There is, therefore, no condition in which we can consider that the model truly represents the condition of the ship with clean surface propelling herself, and in order to put ourselves in a position to estimate the propulsive efficiency in a ship from experiments on her model, our only resource is to undertake a systematic series of experiments on the wake and thrust-deduction in models, aimed at discovering the laws of the effect of variation in skin friction resistance upon amount of wake, and how far, if at all, such variation in wake affects the thrust-deduction also.

A large portion of the experiments upon models and screws which have been recently made at the Torquay establishment have been devoted to this object, and a report to the Admiralty upon the experiments made thus far, is in process of preparation.

For the relatively excessive skin friction of the screw blades on a small scale, a somewhat similar system of correction is also needed before we can treat the measures of the element of screw efficiency proper, yielded by the experiments on model screws, as directly and accurately relevant to the performance of full-sized screws. Assuming, however, that all these systems of correction have been sufficiently determined, or assuming that for the present we consider the measures of efficiency of given screws and models yielded by the model experiments, even without these corrections, a sufficiently valuable indication of the probable efficiencies of the corresponding full-sized screws and ships ; let us now proceed to consider how the system of investigation advocated in this paper may be worked to the best advantage for obtaining results of practical importance.

The work which has generally to be performed by model experiments may be thus described. A design of vessel is required, to fulfil certain novel conditions, and experiments are thereupon made with models of various alternative designs, in order to discover the best design for the purpose. The information yielded by such experiments has hitherto been practically limited to the determination of the relative resistances of such designs ; we now purpose, by aid of screw experiments, to extend the information to the relative propulsive efficiencies. But the total propulsive efficiencies of these designs will depend upon the design of screw with which they are severally to be fitted ; and this is a point which is generally

undecided at the time when the designs are first under consideration, namely, at the time when the model experiments are most required. Even if provisionally decided, the design of screw is generally, in many important respects, open to subsequent reconsideration. Plainly, then, the model experiments should determine the propulsive efficiencies of the proposed designs, not with any particular screw, but with whatever may prove to be the most suitable screw for each ; and they should also be capable of determining what that most suitable design of screw is.

Our object must be to obtain this information from the smallest number of experiments with each model that will serve the purpose. By means of the method of investigation advocated in this paper, and with the aid of certain general preliminary investigations, I think the requisite information may, in most ordinary cases, be obtained sufficiently for practical purposes from a single set of experiments in connection with each model, at one speed, such as the set the results of which are shown in Fig. 4.

The sufficiency of these experiments for the purpose pre-supposes, in the first place, a sufficiently exhaustive general investigation of the subject of screw efficiency proper, to enable us to determine by calculation, without fresh experiments, what will be the efficiency of a screw of any given design, dimensions, proportions, &c., when maintaining a given thrust at given linear speed of progress through the water it works in ; or again, to enable us to determine the design, proportions, &c., of screw that will maintain given thrust at given linear speed, with maximum efficiency. Such an investigation, perhaps, sounds an arduous undertaking ; it is not, however, so formidable as may at first appear, if, at any rate, at this preliminary stage the more minute refinements of shape of blade, &c., may be neglected. The ratio of pitch to diameter, and the number and width of blades, are the characteristics of design on which the screw efficiency proper mainly depends ; and considering that we have not practically to deal with screws of more than four or less than two blades, and that the practical range of variation of pitch ratio and proportionate width of blades is not large, the total number of different models of screws which it will be necessary to try in order to exhaust the subject sufficiently for practical purposes is not very great. The ground has already been in a great measure cleared for such an investigation, by the experiments which have been incidentally made in still water on various model screws.

The sufficiency of a single set of experiments at a single speed, for the purposes above described, also presupposes some general knowledge of the laws of hull-efficiency in respect of (*a*) how it is affected by variations of speed, (*b*) how it is affected by such variations in design or position of screw as are possible in a given design of hull. The experiments already referred to, of which a Report to the Admiralty is in process of preparation, have been in a great measure devoted to the investigation of these points. The general conclusions deducible from this investigation may for present purposes be stated thus :—

(1) The variation of the elements of hull-efficiency with variation in speed is generally slight, and so far regular in its character, that in most cases experiments on a model at about the speed corresponding to the intended working speed of the ship will be sufficient for practical purposes.

(2) Variation of number of blades or pitch ratio of screw does not practically affect the values of the hull-efficiency elements.

(3) On the other hand, variation of diameter of screw, or of its position in reference to the hull, does affect these values, and in a manner which we have not thus far been able satisfactorily to reduce to rule.

The process of determination of the probable propulsive efficiency of any intended design of hull will accordingly take the following form.

Supposing diameter and position of screw to be fixed by circumstances, we have only to make one set of experiments with the model with screw (any screw that we may happen to have, of the correct diameter) in the correct position ; and we shall thus determine the wake and thrust deduction factors (and their product the hull-efficiency) for the proposed shape of hull, and diameter and position of screw. We have then to select, by aid of our previously obtained knowledge on the subject of screw-efficiency proper, the design of screw that will give with maximum efficiency the required thrust at a linear speed equal to the intended speed of vessel minus the speed of the wake. This will be the most suitable design of screw, and the product of its still-water efficiency under these conditions, into the wake and thrust deduction factors, will give the total efficiency.

Supposing that there is room for any large variation in diameter or position of screw, we should (at present at least) be doubtless unable to dispense with other sets of experiments on model and screw, to include the alternative conditions of diameter and position of screw. The cases, however, in which the practical conditions of design admit of large variations in these particulars do not appear to be numerous.

DISCUSSION.

Mr. W. H. White: It would be a very poor compliment to Mr. Froude to pretend that we could possibly discuss such a paper as this after hearing it read, even with his personal explanations. I am thankful to say that I had some previous acquaintance with the principal points raised in the paper, and I followed the general drift of the paper as it has been read, but I do not think I could have done that had it not been for Mr. Froude's previous explanations to me. I am sure of this, that we have in this paper the beginning of a very large work—that is, the beginning so far as practical results go. As Mr. Froude has shown to us, it is really the outcome of a very large work which has been going on, and which I believe I am right in saying was begun by the late Mr. Froude.

Mr. R. E. FROUDE: Certainly.

Mr. WHITE: Almost one of his last on leaving England.

Mr. FROUDE: I think long before that.

Mr. WHITE: I meant in this form, with the present improved apparatus. The investigation has been continued by Mr. R. E. Froude, and amplified in a direction which, I am sure, does honour to him. For these arrangements which we see here in diagrams, when seen in fact display an amount of ingenuity and care, not merely in their design, but in their manipulation, which very few men could possibly give. It is a distinguished honour, I think, my Lord, to this Institution that in Mr. Edmund Froude we to-day have so worthy a successor to his father. As regards the value of these inquiries, I think Mr. Froude will agree with me that, before we can give the utmost practical effect to these model screw experiments, we want some large-scale experiments; just as in the model experiments with ships the late Mr. Froude was so much reinforced by the large-scale experiments which he made in the *Greyhound;* and here, if we can only supplement the model experiments with screw experiments which can be trusted—varying their position, their forms and types, and revolutions—then, what has been often the cause of either great disappointment, or, as Mr. John told us last night, not extreme satisfaction, but which yet remains a mystery, may be cleared up. I may say already that Mr. Froude has helped us extremely at the Admiralty in many points where there was a doubt, in resolving that doubt by means of model screw experiments; and whenever fine forms and high speeds have to be considered, then to be able to make changes in a model which could not possibly be made, except at great expense, in a ship, must be a source of great economy and of great progress in designing. (Hear, hear.)

Mr. C. HALL: I should like to add my meed of praise with reference to this paper. I think that what Mr. White has just said is quite to the point, and I am convinced that this paper contains the elements of a most valuable research with regard to screw propellers. Mr. Froude is well continuing the efforts of his late father in the direction of explaining that which the late Professor Rankine began to investigate in 1865, viz., the relation of V_1 to V. This question has puzzled everyone, but I believe that at last we may entertain some hope of solving the problem practically. It would be impossible, having had the paper such a short time, to discuss it with anything like accuracy; I can only speak of it in terms of praise and admiration. I hope to read it through, and in time to understand it--I confess I do not quite do so at present—and then I have every anticipation of deriving great profit and instruction from it.

Mr. J. H. BILES: My Lord, this paper illustrates, I think, the very pressing necessity there is of getting papers of this kind printed at a very much earlier period in the year. Perhaps I am as much a defaulter as anyone in this respect, in the two or three papers I have had the honour to read before this Institution, but I would like to make a suggestion in a practical form: that the Council should lay down a rule that no paper, from whomsoever it may be received, should be printed or circulated or read at the meetings of this Institution unless it be in the hands of the Council at least a clear fortnight before the commencement of the meetings, and that the papers should be circulated to members in order to give them at least some days to read over, and to thoroughly understand the paper that was to be laid before them. There is no doubt this may be a little difficult to carry out, but the practical effect of the present regulation is this: If a member of this Institution promises to give a paper, he knows that he will be quite safe if he gets his paper in two or three days before the meetings; consequently, when other engagements, which may be

more pressing, present themselves to him, and when he has to decide between these two as to which he shall first give his attention, he says, the paper for the Institution need not be in until such and such a time, and, therefore, it must wait; but if the Council of this Institution were to make it a rule that a paper would not be received if it was after a fortnight before these meetings, then the pressing nature of that engagement would be equal to, if not greater than, some other great engagement; and I think that would have great value in the discussion of these meetings and the science of naval architecture. I speak in that way because I am one of the youngest members of this Institution, and presumably have to look forward to a longer connection with this Institution than most of you; therefore, I am anxious to get that rule made a really working rule as soon as possible. This paper, my Lord, adds very considerably to the inducement for private shipbuilders to take up this question of model experiments. Model experiments at present have been few, and though they have been of value, they have not been of sufficient value to induce shipbuilders to take up the question. We decided a few years ago to lay down a tank. Circumstances interfered in the shape of a fire in the establishment, which disorganised the place for some time, and we have not been able to undertake the construction of that tank; and until hearing Mr. Froude's paper, in giving my opinion on the question I have not been able to advise the firm to undertake that large task for the simple reason that experiments with models alone do not cover sufficient of the ground to make them of the value that the money laid out would lead one to suppose they would. But this paper of Mr. Froude's adds so very largely to the measure of efficiency that one can determine by model experiments in relation to steamship trials, that I think really now the question has become a question for more than one shipbuilder—it has become a question for many shipbuilders; and, if I may be allowed to make the suggestion, it appears to me that it is almost a question now (of course, assuming that Mr. Froude's deductions in this paper are correct) for a body like Lloyd's to take up. They have taken up many questions in connection with shipbuilding, and I suppose one may fairly say that they have taken them up successfully; and it appears to me this is one of the questions they might take up. Mr. White referred to experiments with large screws, but where could you have better experiments with large screws than can be had day by day with ships of the Mercantile Marine, if these experiments were properly organised by some body like Lloyd's. If Lloyd's were to undertake the determination of the resistance of models and the efficiency of screw propellers, the experiments with full-sized propellers could go on day by day. There are sufficient trials made to enable one to make in a very short time a number of experiments, and to obtain the results with but very little extra cost. Therefore, I think that this is a subject that might be considered when the members of Lloyd's Committee have sufficient time at their disposal. I may be wrong in proposing it, and perhaps am a little too enthusiastic on the subject, but it appears to me, with the appreciation I have—perhaps it is too much—in favour of this subject, it is one that cannot be attended to too soon, because it involves such very large interests, and is really too big a question for one shipbuilder. With all respect to Messrs. Denny and some of the large shipbuilders, their experience in connection with steamship trials is, after all, very limited. They only turn ships out at the rate of about one in the space of two months, whereas the Mercantile Marine of this country turns out at least a ship every day, and therefore the opportunities for investigations of this kind would be very much increased if the subject were taken up by a body like Lloyd's Committee, and trials were properly organised. Then this paper also shows how very dangerous it is to act upon the results deduced from experiments which are made upon limited assumptions. Now, two or three years ago the late Mr. Froude read a paper here. It was a well-defined paper. On certain assumptions certainresults were deduced, but the outcome of it in some respects was this, that many people who were connected with carrying

out practical work ignored, to a large extent, the assumptions, but adopted the results deduced from those assumptions. I think I am within the mark when I say that one of the tendencies of Mr. Froude's deductions was this, that propellers were too large and had too much surface. The consequence was, that a great many people reduced the diameter of propellers and reduced the surface. There have been several lamentable results due to this, and therefore it would be well that the profession in general, on reading this paper, before commencing to act on the results which may be deduced (I am not quite clear whether there are any results pointed to in this), to keep clearly in mind that fact—that, in addition to the results deduced, one must be thoroughly familiar with the assumptions on which they are deduced. I have in my mind the case of a ship of 5,000 tons, with a power of 4,000 indicated horse-power. In order to develop the horse-power easily on a small weight of machinery, and guided, to some extent, in that resolve by Mr. Froude's observations, the propeller was made some 17 feet in diameter. I think the members will agree with me that 17 feet of diameter for a vessel of 4,000 horse-power and of 5,000 tons gross, appears to be small: the speed of the ship was about 14 knots. The consequence was that that vessel did not attain as high a speed as would have been obtained with a larger propeller. The result appeared to be a good one, and the owners were quite satisfied; but the next ship which followed was a ship of 1,300 tons more displacement. She was of the same length, but she had 3 feet more beam and on trial had 3 feet more draught. Her propeller was exactly of the same pitch, and about 3 feet more diameter. That is in opposition almost directly to the results deduced by Mr. Froude; and that ship, in spite of her having, I think, 1,700 tons more displacement, attained exactly the same speed for the same power. The propeller of the second ship was not too small, and the mistake which was made in the first case was in assuming that a propeller could not be made too small. I wish it to be understood I do not wish to detract in the least from the results obtained by Mr. Froude, because I have the highest appreciation of them; but what I wish to point out is this, that one should not deduce practical and direct results from the results which he deduces on certain well-defined assumptions unless these actually apply to the particular ships in which the results are being tried. Then there is this further question in connection with that. Mr. Froude's experiments are always made on the assumption of an unlimited amount of power available—that is to say, that the amount of power which he has to drive his screws is unlimited; but there is always a point in an actual ship where the power is limited, and that is the safety valves of the boilers. You cannot increase and decrease pitch, and determine absolutely the efficiency of a propeller in a ship like you can in the case of a model screw; because, although you may increase pitch and get greater efficiency, the engines of the ship are so formed that they will not take in more steam than the parts will admit. The consequence is that, although you may get higher efficiency as far as the screw propeller is concerned, you may not be able to develop the power which the boiler is able to develop, and, consequently, that is an element of great practical importance, which does not enter into the theoretical value of the efficiency of the propeller, but which in any question of altering the shape of the propeller, or affecting the design of a propeller in a new ship, must be taken into consideration. I must thank Mr. Froude for the very able and clear way in which he has placed this paper before us, and I only regret that the members of this Institution had not a proper opportunity of thoroughly digesting it before the paper was read, in order that they might not only have criticised it, but have added in a much higher degree to the praise which should be bestowed upon Mr. Froude for his paper.

Sir Edward Reed: My Lord, when you put to the meeting the question whether an extension of time should be allowed Mr. Froude, I felt, and I believe the meeting also felt, from the vote they

gave, that this was signally a case in which the author might be allowed greater latitude, because it could not be expected that the meeting would be able to enter into a minute discussion on such a paper. I rather think Mr. Biles, in his latter remarks, has been criticising some other paper, not the one we have heard read this evening, because this paper is remarkably free from any possible element calculated to mislead anyone, and is entirely devoid of anything like even a suggestion of proportions, or anything whatever as to the means of determining upon the size of screws. This paper, so far as the Institution and the professional world is concerned, although not so far as the results of previous labours are concerned, is the commencement, as Mr. White well put it, of a very large subject. It seems to me to be the first considerable inroad into that desolate region of ignorance which besets our profession. I do not know any question in connection with naval architecture in which we are so much at fault as with regard to the determination of the screw-propeller in relation to the ship; and I have myself, at the present time, a difficulty with two ships which have been conditionally purchased. I am perfectly certain that if I could make a few experiments, wisely resolved upon, I could determine some points which with the ships themselves it is almost impossible to determine, because every time you make an experiment you spend a couple of hundred pounds, and you cannot afford to make expensive and multiplied experiments when you have to pay for them in that manner. I wish to be allowed to say a word or two in praise of this paper, for this reason. It goes a little against the line which Mr. Biles took in the latter part of his remarks, but he will not mind that. I was afraid, to tell the truth, when I heard Mr. Froude open this paper, looking at it for the first time, that I was going to lose myself very soon, because of the probable fact that a number of conditions and assumptions would begin to enter in, which would defeat my interest, and make me willing to wait for a very long time before being able to turn the results to a practical account, because that is the effect, generally speaking, in connection with experiments bearing on naval architecture. Not unfrequently, I may say, with regard to calculations and theories touching naval architecture, so many assumptions are introduced one after the other, that by the time you get to the end of them you have lost confidence, and are not willing to go forward on those lines. I noticed particularly the manner in which Mr. Froude discussed the composition, if one may so call it, of the wake, and the thorough recognition which he gave to the practical differences between the assumed wake of a constant stream and the actual wake of a turbulent stream. His recognition of those differences was so thorough, and his references to them so instructive and encouraging, as to the integrity, if one may so speak, of his investigation, that I feel peculiar pleasure with it; and I am satisfied that this paper, after it is on the records of the Institution, will be a most valuable production—in fact, we have had no more valuable, in my opinion. With regard to what Mr. Biles said about the production of papers, I do not think we ought to attempt to draw such a large deduction as to say that no paper should be admitted unless it is presented within a fortnight before the meetings are held. Of course it would be an immense advantage—it would have been to-day with Mr. Watts' paper—if we had had it in our hands before the meeting; and such a paper as that we might fairly claim to have had before; but with regard to a paper of this kind, I apprehend there would never be any close discussion, because there would not be much to discuss in it—all that is to be hoped for would be a few suggestions here and there. What I want to point out is, that if we cannot discuss the subject-matter of this paper, we get it in our hands and on the record, and what is the consequence—that, probably, we shall have a paper by Mr. Biles on the same subject next year, and we shall get a succession of papers on the development of this question, whereas perhaps otherwise the commentators upon this paper would have been satisfied with a passing criticism at this meeting; so that the circumstances under which the paper

has been presented are not wholly without their compensation. At the same time, I think the Council might profit by Mr. Biles' suggestion, so as to press strongly for papers generally coming in a little earlier, and getting into our hands a little sooner. I do not know that I can say any more, except to confirm what Mr. White says. It is to my mind a matter of the greatest possible satisfaction, and I am quite sure it is to this Institution, to know that Mr. Froude is doing such thoroughly valuable work. In this he is following upon the labours of his lamented father; he is feeding the future profession very largely, and I earnestly hope that nothing will be said or done on occasions like this to discourage him in the great and valuable work which he has in hand.

Mr. Biles: You will allow me, my Lord, one word in explanation. Sir Edward Reed says that I was criticising a former paper.

Sir Edward Reed: I said you were not criticising this one.

Mr. Biles: I believe the presumption from that is that I was criticising another one. I wish to make it quite certain that I have nothing but commendation for the earlier paper of Mr. Froude's, but what I do want to impress very forcibly upon the members, is that any deductions which may be drawn from this shall be drawn with a full knowledge of the assumptions upon which they are made. Sir Edward Reed has corrected me, and said that the assumptions are practically nothing.

Sir Edward Reed: I have not said that.

Mr. Biles: That only gives force to what I say, that the papers should be earlier in the hands of members, in order that the time of the meeting may not be taken up by anyone making a statement such as I have made, which would not have been made if I had seen the paper earlier.

Mr. W. John: I would like to say a word or two, and I can endorse every word that fell from Mr. White and Sir Edward Reed as to the extreme value of this paper to a naval architect. Sir Edward Reed put the matter very cleverly when he said it opens out a hope of our getting from a state of ignorance into something like a decent state of light on the subject of screw propellers. We know a good deal of the theory of naval architecture in many directions, but on this one question of screw propellers, I may say I don't know a naval architect who is not in as near a state of absolute darkness as it is possible to be in. Now, of course we can work tentatively from one ship to the other where the steps are not great, but as for knowing the absolute theory of the thing, and as for being able to argue in anything like a bold way with large steps, I think the whole of us are almost adrift, and hopelessly adrift, and I do welcome the fact of Mr. Froude taking up this question in the bold manner he is doing, in the spirit in which his father attacked some other questions in naval architecture that appeared insoluble, and mastered them. I do feel more pleased that this is the particular question Mr. Froude has taken up at the present time than if he had taken up any other question in connection with the subject. Mr. Biles suggested probably Lloyd's would take up the matter as a means of feeding Mr. Froude with the records of the Mercantile Marine, and although I do not think it is probable Lloyd's Committee would see their way to do so, I do think there is another way of doing it, and that is that the private shipbuilders in the country should put their data and the result of their experiments and their trials freely in the hands of a man like Mr. Froude, and let him deduce what he can from them, and check them by his own model experiments, and in that way we can get together in a comparatively short space of time an amount of data that will enable Mr. Froude really to settle this question, or at any rate to put it on a sound basis. Of course there comes the question whether private

shipbuilders will place their data in the hands of anyone outside their own firms. Some builders are prepared to do it, and some do it freely at the present time; those who don't see the advantage of doing it in a case of this kind, will simply not be able to appreciate the results when Mr. Mr. Froude has solved the question. And, I say, while fully recognising the desirability of competition between private shipbuilders, I believe that it would be better for all of us to compete in light than to compete as we are at present, in darkness, and therefore it would be to the interest of all shipbuilders in the country to give Mr. Froude, as freely as the Admiralty give him now, all our data, with only one condition, that his results shall come out publicly at Institutions like these, as I know they will sooner or later, and that it should be a fair open attempt all round to get at a solution of the most difficult and most complicated thing at present in connection with naval architecture.

The PRESIDENT: I must take the opinion of the meeting now as to whether you would wish me to close this discussion. I must point out to you that the original reading of the paper occupied fifty minutes or more, and it was thought that that latitude should be given, because the discussion itself would be a very short one. We have three papers more to be read, and therefore I must ask you to express in some mode whether you wish the discussion to go on or not.

It having been put to the meeting as to whether the discussion should be closed,

The PRESIDENT: I see that the feeling of the meeting is that it should be closed, and therefore I most unwillingly close the discussion.

Mr. R. E. FROUDE: I have very few remarks to make in reply, except to thank all the gentlemen who have joined in the discussion most cordially for the undeservedly complimentary strain in which they have spoken, and I can only say that I am very glad that the paper has given rise to so valuable a discussion, if not on the details of the subject dealt with, on the very important general question of the use of scientific investigation, that is to say, the manner in which it is to be used for practical purposes in naval architecture, and those remarks have suggested to me one word from myself in reference to the scope of this paper. It certainly professes to suggest a manner in which the work which is now done by experiments on models in reference to the question of resistance may be extended to the question of propulsive efficiency. It professes to suggest a way which, I think, is the only way of doing it, if it can be done, though I am not as yet certain that it can be done very effectively or successfully. The corrections which may be required to make model screw experiments fully applicable to full-sized ships may really baffle us; but I feel that, even if we are baffled in that attempt, this paper has a value, in that the propositions which I have put forward are a contribution to the science of the subject, and, I think, in virtue of the light thrown on the question by the propositions, we shall be able to do a good deal in analysing and systematising the results which are to be obtained from full-sized trials with ships; and that we can thus make much better use of results obtained from steamship trials than can be made without the light which is thrown on the general question of propulsive efficiency by this investigation. (Hear, hear.)

The PRESIDENT: Those sounds are quite sufficient, and I am sure I may convey to Mr. Froude your most grateful thanks for his very valuable paper.

THE SPEED AND FORM OF STEAMSHIPS CONSIDERED IN RELATION TO LENGTH OF VOYAGE.

By J. Hamilton, Jun., Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

This somewhat comprehensive title might cover a great deal more than I propose to bring before the members of the Institution, and I must beg you to remember that in what follows I shall refer only to vessels whose earnings are made for the most part, or entirely, by carrying dead-weight cargoes. The conclusions arrived at would apply, more or less, to passenger vessels or ships carrying light cargoes; but, to keep the paper within the prescribed limits, I confine myself to the necessity there is for running cargo vessels at suitable speeds, and of designing new cargo ships of suitable forms, in both cases with a proper regard to the length of the voyage to be performed.

It is no part of the paper to deal with that closely allied subject, namely, the relative commercial efficiency of large ships, *per se*, as compared with smaller ones. This is, I believe, however, of small moment, as compared with the importance of studying the relation that exists between the speed and form of steamers generally, and the service that the vessels are intended for.

There is nothing, I imagine, that gives the steamship manager more care and thought than the matter of coal supply, and there is certainly nothing that the naval architect has to deal with in designing ships that asserts its importance more than the fuel question. If coal weighed only half what it does, what possibilities there would be in steam navigation. It is possible that a refining process may some day be discovered, and the weight of coal reduced; but in the meantime, and taking coal as we find it, it will be profitable to realise to the fullest extent the disadvantages of having to carry the required supplies, so that the necessary evils may be reduced as much as possible, and the best commercial results realised. The effect of different rates of consumption on a ship's carrying power is two-fold. First, there is the difference in the cost of the coal burned; and, secondly, the difference in freight received for cargo carried. The comparisons must therefore take a commercial shape, and I have attempted to look at the matter from the shipowner's point of view, and for this purpose the accounts relating to an actual voyage have been

carefully analysed and put at my disposal by a steamship owner interested in these matters.

The accounts referred to belong to a voyage made by a vessel that was tried progressively on the measured mile, and, therefore, the power required to drive her at different speeds can be correctly deduced.

Taking the question of speed first, the intention is to show that the most economical speed at which to drive a vessel should be regulated by the length of the voyage. For instance, a certain speed may be very suitable for one voyage, but ruinous for another of different length. I fear, however, that this matter of economical steaming is so mixed up with questions of policy that it gets lost sight of. In many trades, the seasons and markets regulate this matter; and then there is the inveterate love of racing at sea, which goes to increase the difficulty of regulating the speed. These things, and perhaps the difficulty of securing cargo, unless there is at least the promise of a quick passage, must often prevent proper attention being paid to this all-important matter; but there must be a large portion of our carrying trade done by steamers under the same conditions only as are imposed on sailing vessels, and where there is consequently some latitude in the choice of speed. In any case, whether expediency overrules economy or not, it is well that the relation existing between speed and length of voyage should be clearly understood.

The voyage referred to was made at the rate of 12 knots all the way, and I purpose to show the effect that reducing the speed of the same vessel to 10 and 8 knots respectively has in increasing the profits.

But at this point I must say that there may have been, I have no doubt there were, considerations that determined the owners to make this particular voyage at 12 knots. I merely take this voyage to illustrate what is found, I believe, daily, and when no other considerations than those of economical cargo-carrying have a part in regulating the speed. The neglect to consider the relations between speed and length of voyage is, I believe, sufficient to account for much of the chequered experience, as regards dividends, of shareholders who invest their money largely in sixty-fourths.

You will therefore, I hope, bear with me whilst I explain the basis on which the comparisons that follow are made.

The items in the accounts referred to have been split up into six divisions, as follows :—

(1) *The freight or revenue.*—The quantity of cargo that can be carried is always taken as the total dead-weight carrying power, less the weight of coal required for the whole voyage.

(2) *The cost of coal burned on the voyage.*—The quantity of coal required per day is taken as being in direct proportion to the power developed in driving the vessel, and a

fair reserve supply is added, one-third being allowed for 12 knots, two-fifths for 10 knots, and one-half for 8 knots, on the principle that the time during which the vessel would be exposed to retardation, due to strong winds, would be in about the proportions of 4, 5, and 6.

The voyage referred to took 27½ days, and the coal burned was at the rate of 43½ tons per day, giving 1,200 tons for the voyage, 400 tons reserve coal, and leaving 1,800 tons for cargo.

At 10 knots and 8 knots speed the voyage would extend over 33 days and 41¼ days respectively; and the coal per day taken in proportion to the power corresponding to the speed will be 26 tons for 10 knots, and 13 tons for 8 knots. At 10 knots, the coal required for the voyage will therefore be $33 \times 26 = 858$, and taking 342 for reserve, 2,202 tons are left for cargo. At 8 knots, the coal required would be $41\frac{1}{4} \times 13 = 536$, and with reserve of 268, leaving 2,606 tons for cargo.

(3) *Oil waste and engine-room stores.*—This item is supposed to increase or diminish in proportion to the power developed by the engine, or as the coal burned, which would be a measure of the heat produced, and therefore of the oil, &c., used.

(4) *Lights, dues, pilotage,* and all expenses of this nature, which are chargeable each voyage, and depend on the size or tonnage of the vessel, and not on the cargo carried.

(5) *Cargo dues, stevedoring, brokers' commissions,* and all similar expenses which depend on the cargo carried. This is assumed to be always in proportion to the freight, the rates being regarded as constant.

(6) *Wages of officers, engineers, crew, &c., victualling and provisioning the ship,* and all expenses belonging thereto. This is assumed to be a fixed sum for a given length of time.

The number of voyages that would be made in a given time would be as the speeds, namely, as 12, 10, and 8, if the time spent in port followed the same proportion, but this will be regulated by the dead weight put in or taken out. In loading, the total weight being the same in all cases, the same time will be taken up per voyage, but in discharging, only the cargo is put out, and the time in port will vary as the quantity. Allowing, therefore, a day for every 426 tons put in or taken out, we have 8 days to load and 4½, 5, and 6 days per voyage for discharging. This added to the steaming time gives 40, 46, and 55¼ days required per voyage, at 12, 10, and 8 knots respectively. Or in 360 working days say 9 voyages, 7·8 voyages, and 6½ voyages.

Making use of these figures, then, and basing the comparison on the principles already described, I have worked out the comparative earnings for speeds of 10 knots and 8 knots, and place these alongside of the figures relating to the actual voyage.

The total dead weight was 3,400 tons.

TABLE I.

12 Knots.	10 Knots.	8 Knots.
Steaming time .. .. .. 27⅓ days. Coal, 43⅓ tons per day.. .. 1,200 tons Reserve .. .. 400 ,, 1,600 ,,	Steaming time .. .. .. 33 days. Coal, 26 tons per day .. .. 856 tons. Reserve .. .. 342 ,, 1,198 ,,	Steaming time .. .. .. 41⅓ days. Coal, 13 tons per day .. .. 536 tons. Reserve .. .. 268 ,, 794 ,,
£ (1) Freight 1,800 at £3 5s. = 5,850	£ Freight 2,202 at £3 5s. = 7,156	£ Freight 2,606 at £3 5s. = 8,470
£ (2) Coal, 1,200 at 12s. 5d. = 745	£ Coal, 856 at 12s. 5d. = 531	£ Coal, 536 at 12s. 5d. = 332
(3) Oil 88	Oil 62	Oil 39
(4) Lights, &c. 415	Lights, &c. 415	Lights, &c. 415
(5) Cargo dues 706 1,954	Cargo dues 817 1,825	Cargo dues 960 1,746
3,896	5,331	6,724
£3,896 × 9 = 35,064	£5,331 × 7·8 = 41,581	£6,724 × 6½ = 43,706
Less item (6) Wages ... 15,714	Less 15,714	Less 15,714
£19,350	£25,867	£27,992

The results of this comparison are represented graphically by the curves in Fig. 1, p. 260.

In Fig. 1, the gross earnings in a time corresponding to 9 voyages at 12 knots, are set up from the base E F, and a curve drawn through the points at 8, 10, and 12 knots respectively, giving the line A, B, C. From the base is also set up the constant expenditure of crew, &c., represented by the straight line G, H. On the top of this are set up the sums under item (5), or cargo dues. Above this the items under (4), or the light dues. Then the expense in oil and coal. The gross earnings are bounded by the line A, B, C, and the gross expenses by the line J, K. The distance between these lines at any speed represents the profit.

You will observe from the diagram that the distance between the two lines is greatest about 8 knots, so that the balance of earnings over expenditure is greatest at this speed, and it may surprise you to find that it is about 43 per cent greater than at 12 knots, the speed at which the voyage was made.

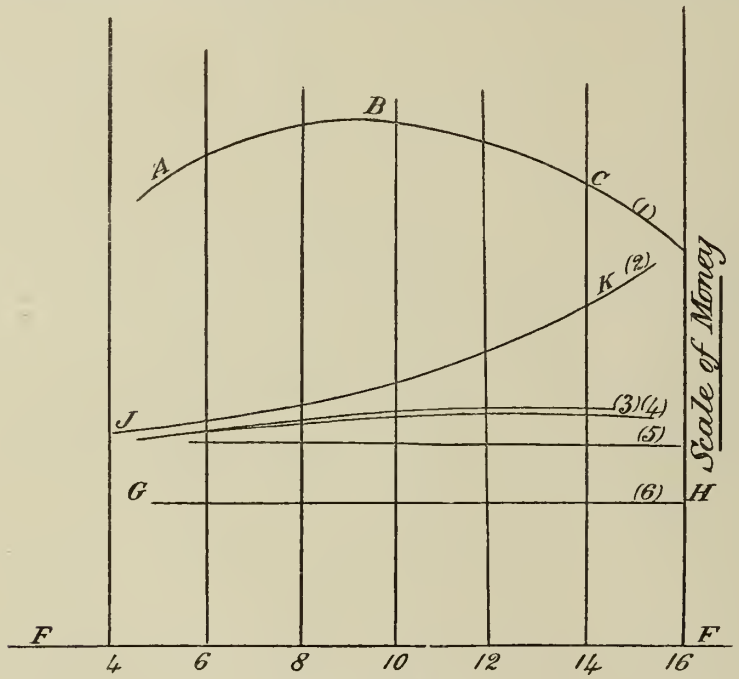
We see, therefore, that it will pay to reduce the coal, thereby increasing the cargo carried even at the expense of speed; or, to put it more plainly, that more cargo will be carried with 6½ voyages in the year at 8 knots, than with 9 voyages at 12 knots. At the same time terminal port charges, cost of coal, oil, &c., &c., will be much less.

Here then is an example of a vessel, taken from actual practice, which is being driven at a speed too great for economical steaming on a voyage of 8,000 miles, and we find that the earnings in a given time would be increased by reducing the speed considerably on such long voyages.

Having started with an actual ship, and compared the effect of reducing coal at the expense of speed, I now propose to apply the same method of comparison, and indeed the same rates or charges, excepting the rate of freight, which I alter to correspond with

— Fig. 1. —

Curves of Revenue and Expenditure taken for 360 *Working Days.*



Scale of Knots.

(1) Freight.
(2) Coals.
(3) Oil, Waste, &c.
(4) Light, dues, &c.
(5) Cargo dues, &c.
(6) Wages, Victualling Ship, &c.

the distance, to voyages of 4,000 and 1,000 miles respectively. As a matter of course, the variations in profits turn largely upon the quantity of cargo shut out to provide for the coal supply, and as this quantity bears a constantly decreasing proportion to the cargo carried as the distance to be provided for is reduced, what applies to a long voyage may not apply to a shorter one.

A voyage of 4,000 miles would be accomplished by the vessel referred to, at 12, 10, and 8 knots respectively, in $13\frac{3}{4}$ days, $16\frac{1}{2}$ days, and $20\frac{3}{4}$ days. The total dead-weight of

coal and cargo being 3,400, the same in each case, the time required to load will be the same, namely 8 days, and to discharge, 6·1, 6·6, and 7 days. In 360 working days the number of voyages would therefore be 13, 11½, and 10.

Taking the terminal port charges the same as before, cargo dues, &c., in proportion to the cargo, coal at the same rate as before, and the freight at £1 15s. per ton, a rate somewhat in proportion to the distance, the result is that the sum left for profit and insurance would be for 12 knots £26,861, for 10 knots £26,974, and for 8 knots £25,356.

In the same way, for a voyage of 1,000 miles, if the rate of freight is taken at 20s. per ton, the results would be £28,309, £28,409, and £26,820, for 12, 10, and 8 knots respectively.

These figures, along with those found for the voyage of 8,000 miles, are represented graphically by the curves on Fig. 2.

It will be seen from the diagram, that for voyages of less than 4,000 miles the curves representing 12 and 10 knots curiously coincide, but are higher than the 8 knot curve.

On 6,000 miles it would be indifferent whether the speed were 8 or 10 knots; possibly 9 knots would be the best, although both are better than 12 knots; but when 6,000 miles is exceeded there is a difference, and it constantly increases.

You will see that in an Australian voyage, say of 12,000 miles, the profits would be doubled by running the vessel we are considering for any given number of voyages at the speed of 8 knots per hour instead of 12 knots, the rates of freights, dues, cost of coal, &c., being the same.

I do not know if shipowners realise, that what may be the most economical speed for a short voyage may be ruinous for one of twice the length; but, on account of the necessity to provide coals for the whole voyage, it is so. The common practice, however, is to try to evade this necessity by converting a long voyage into two or more shorter ones, by calling at ports on the way, when possible, to re-coal. It is not always possible, however, and is at best a doubtful expedient, as can be easily shown.

For example, this vessel would carry 800 tons additional cargo if only half the coal had been put on board, which at £3 5s. gives £2,600 additional freight; but 1½ days would have been spent in coaling, or 13½ days in the year, which is equal in time to one-third of a voyage.

The profits on one-third of a voyage, in this case £1,415, would be lost. The difference in the price of coal at the intermediate port might be 21s. per ton, or on 800 tons £840.

Then port charges and cargo dues would be incurred, so that even with freight that leaves a good margin there is not much to be gained by breaking the voyage for the purpose of re-coaling.

Of course a shipowner would be guided by the relative rates of freight, and cost of coal, but I believe in the ordinary cargo-carrying ship it will pay better, when the distances are very long, to run at slow speed without stopping to re-coal.

Questions of expediency will always have a large share in regulating this matter. What we have found amounts to this, that in the vessel we have been considering the necessity to keep down the consumption of coal is comparatively small if the voyages are short, but that for long voyages it is imperative to secure a small consumption of coal at any cost; and that the speed that is too costly for long voyages may pay best for short voyages.

The most economical speed, however, at which to drive a ship for any length of voyage differs in different types, so that the relations existing between speed and length of voyage must be studied for every separate and well-defined type of ship. Twelve knots' speed, for instance, although too costly to maintain in the vessel referred to, might be the right speed for a vessel of other form and proportions.

To illustrate this, I have applied the same rates to four types of ships, of which the speed and power capabilities are known. As you may have observed, the voyage we have selected from practice left the owners with a very handsome profit, and it will not affect

the comparison, for better or worse, if we assume, in dealing with other vessels, the same prosperous state of matters.

Table II. gives the particulars of these forms.

TABLE II.

	Dimensions.			Displacement.	Ratio of Length to Breadth.	Co-efficient of Fineness.	Coal per Day at 2 lbs. per I.H.P. per Hour.		
	Length.	Breadth.	Draught.				8 Knots.	10 Knots.	12 Knots.
A.	328·7	35·1	20·05	4,200	9·36	·635	8 tons	15·5	27·1
B.	309·5	38·2	20·40	4,200	8·10	·610	8·4	16·7	29·5
C.	286·0	35·1	19·95	4,200	8·16	·734	10·1	20·4	39·83
D.	269·8	40·8	18·90	4,200	6·61	·708	13·6	24·4	48·40

The vessels were all carefully tried on the measured mile, but at lighter draughts of water, and the indicated horse power for the load-draught of each has been taken as in proportion to the $D^{\frac{2}{3}}$. A slight correction has also been made by the aid of the late Mr. Froude's law of comparison, to bring all the ships to the same size as measured by displacement.

They present four types of steamers which are not the least unusual in form, but are frequently met with.

The proportions of length to breadth vary considerably, as also the co-efficient of fineness. These ratios are given in the Table, and when taken together give a fair idea of the relative fineness or fulness of the respective ships. In the last three columns is given the coal required by each type at different speeds.

The trials were, of course, made under the best conditions of weather, and when everything was new, and therefore the figures given in the Table for coal can only be used correctly for comparative purposes, but are perfectly accurate to illustrate the matter in hand.

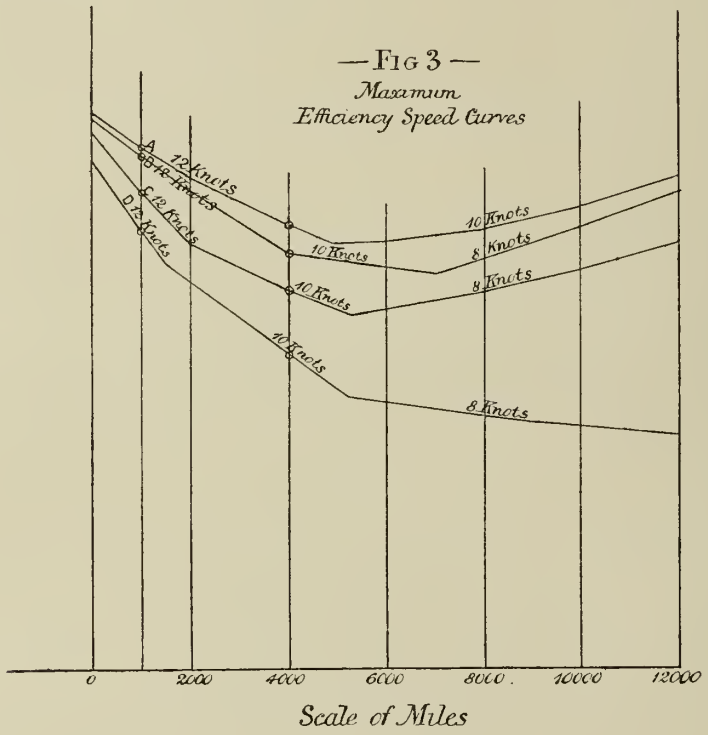
On the same principles as previously stated, the returns that each of the vessels given in the Table would make on voyages of 1,000, 4,000, and 8,000 miles, have been calculated, taking the same rates of freight as before, and coal at 10s. per ton. To give the figures in detail for this would only encumber the paper, and I have therefore represented the results graphically in Fig. 3, p. 264, and to make the diagram less complicated, have only shown the part of the different speed curves which, for the distance, represents the maximum efficiency.

For the vessel marked A, 12 knots would be more economical than 10 knots for voyages up to 5,000 miles, but for longer voyages 10 knots would pay better.

For B, the best returns would be got by running on voyages under 4,000 miles at 12 knots; between 4,000 and 7,500 at 10 knots; and above this at 8 knots.

For C, on short voyages under 2,000 miles, 12 knots is best; for voyages between 2,000 and 5,200, 10 knots is most economical; and for longer voyages, 8 knots.

For D, 12 knots, under 1,400 miles; 10 knots for 1,400 miles to 5,200 miles; and 8 knots for longer voyages.



Always provided that the revenue is derived by dead-weight carrying alone.

Of course there would be points at which the intermediate speeds would prove best, but this diagram will show in a general way that on a voyage of any given length a speed that is unsuitable for the distance for one type may be quite suitable for another, and it is obvious that a shipowner who understands how to regulate the speed of different ships with due reference to the lengths of the voyages they are employed on, may make a fortune, whereas the steamers of the less intelligent owner may have enough to do to make ends meet.

It must have occurred to many of you that to keep down the quantity of coal consumed at the expense of the speed is a mixed blessing. To carry a large amount of

cargo cheaply per voyage by reducing the coal, has great drawbacks, if it is accompanied by a corresponding reduction in the speed, and consequently of the number of voyages that would be made in a given time. And yet I believe that the practice, especially with the smaller class of ships, is a very common one, to put a small quantity of coal on board and leave the captain to make the passage with it. This may be making the best of a ship ill adapted for the length of the voyage, and I have shown that it may pay to reduce coal supply even at the expense of speed; but how much better it would pay if ships were always built of suitable proportions and form for the length of voyage or service in which they are to be engaged. The lowering of the speed is a necessary evil, and it unfortunately goes a long way to neutralise the twofold advantage of a small consumption.

This brings me to the second part of the paper, the subject of which is the relative commercial efficiency of different types of vessels.

I propose to illustrate the differences in commercial efficiency, considered in relation to the length of voyage, due to the proportions and forms that, as a matter of fact, really exist between steamers, for in dealing with long voyages especially it becomes not only advisable, but imperative, to secure the minimum of coal-consumption without sacrificing speed.

You may have remarked on Diagram 3, that as regards commercial efficiency the order of merit is just the order in which they are named, A being first and D last.

It is in accordance with general practice to consider a fine ship as better adapted for long voyages than a full one, but I think it will surprise some of the members to find that in the vessels I have put before you, the order of merit is the same for any length of voyage; that is, the vessels that are shown by the diagram to be superior in money-making capabilities for long voyages, are also superior on the shortest voyages. This is not generally understood. I propose, therefore, to show the values of A, B, C, and D relatively to the length of the voyage to be performed, and to each other, and also to point out within what narrow limits the commercial superiority of very full ships is confined.

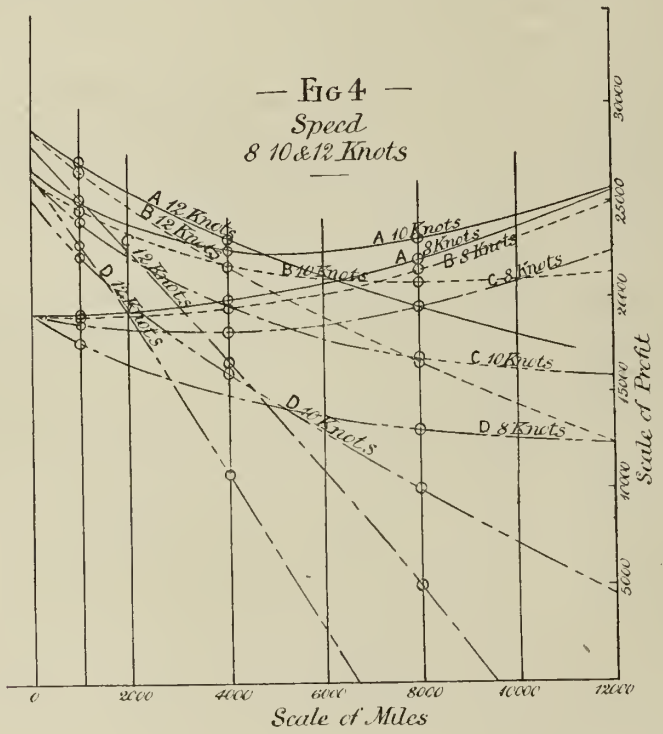
This part of the subject can be illustrated by the curves already given in Fig. 3, but completed (see Fig. 4, p. 266).

It will interest you to know that the difference in money-earning power, even at such a low speed as 8 knots, is, between A the most efficient and D the least efficient, quite equal to the difference that we find existing between one steamship company that makes and divides handsome dividends, and another whose operations result in periodic calls being made on the shareholders. On a voyage to Australia, for instance, it might amount to a difference of 100 per cent. At 10 knots' speed the sharp ship would earn five times that of the full one, whilst at 12 knots speed A would have a handsome sum at her credit whilst D would have a like sum on the wrong side of her ledger. At even the very slowest speeds the fine forms are superior to the fuller types.

From the table it will be seen that as regards A, the length is 9·36 times the beam with a co-efficient of fineness of ·635, whereas of D the length is only 6·61 times the beam with ·708 as co-efficient of fineness, the displacement in each case being the same. The other vessels take up intermediate places, and the power required to drive them, or the coal required, is just about in proportion to their fineness or fulness. A fair idea of this can be gathered by comparing the ratio of length to breadth, and co-efficient of fineness, taken together; or, the displacement being constant, the power is very nearly in proportion to

$$(B + d) \times \text{co-ef.}$$

or the sum of the beam and the draught multiplied by the co-efficient of fineness.



It may seem paradoxical that the full ship D is inferior to A even for very short voyages, but it is capable of very easy explanation.

These vessels are compared at 12 knots, and consequently an allowance is made in the weight for machinery, capable of driving each just 12 knots, loaded, in smooth water. Now, comparing for example A and D, the weights are as follows :—

	A.	D.
Weight of hull	1.571	1,449
Do. of machinery for 12 knots ...	234	418
	1,805	1867

Here the extra weight of the engine and boilers of D exceeds the extra weight of the hull, due to the larger dimensions of A, and therefore D would carry less cargo, apart altogether from the coal supply. This is an extreme case, and a comparison between B and C will better illustrate the matter.

The weights are as follows :—

	B.	C.
Weights of hull	1,557	1,466
Do. of machinery for 12 knots ...	255	344
	1,812	1,810

Which makes the capacity of C slightly superior to B; but as the daily consumption is respectively 10 tons and 8·4 tons, the superiority would only hold for voyages of a length requiring less than two days' coal to be put on board.

If, instead of for 12 knots, machinery for a very moderate speed were put in each ship, a full ship might be superior to a finer one; for instance, if we take 8 knots for 400 miles, the weights would be as follows :—

	A.	B.	C.	D.
Weight of hull	1,571	1,557	1,466	1,449
Do. for machinery for 8 knots at sea	81	86	104	140
	1,652	1,643	1,570	1,589

The displacement being in each 4,200 tons, the dead weights will be—

	2,548	2,557	2,630	2,611
Coal supply for 400 miles at 8 knots, with reserve of $\frac{1}{3}$	19	20	26	36
The results are	2,529	2,537	2,604	2,575

The carrying capacity of the two fuller ships is certainly superior to that of the two finer ones, and the earnings would be represented in the same way as before by the curves in Fig. 5, p. 268.

B and C are superior to A at 4,000 miles and under.

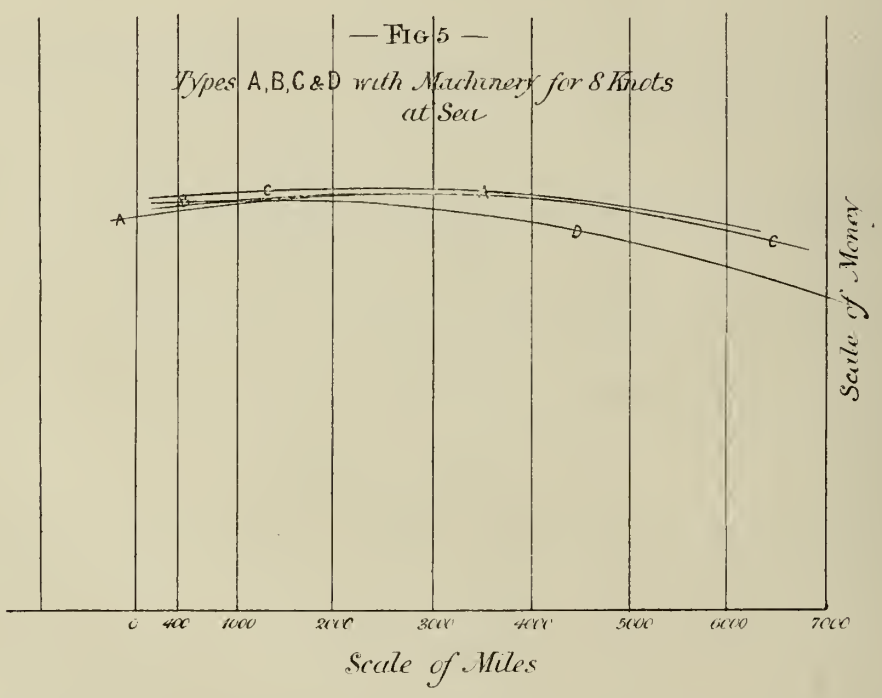
D has the advantage over A at 1,300 miles. It must be remembered that this is only so if no superfluous weights are carried, and on the assumption that machinery which is capable of driving each vessel at 8 knots and no more is put on board.

It is also worth while to point out that superiority confined to such low speeds may be more apparent than real, as we found that it pays better to run for voyages of this short length at comparatively high speeds, and the finer types would therefore have the advantage. (See Fig. 4)

As regards the *costs* of these respective types, the hulls of larger dimensions, although of the same displacement, will cost more than the shorter, fuller vessels; but against this

has to be put the smaller costs of engines required for the sharper vessels. Whether one ship will cost less or more than another will depend on the speed that is taken, but it may be said generally, that for ordinary speeds the vessel with the small consumption, but doing the same work, can be had at the same cost as the more wasteful steamer.

Some of these statements may be new to our shipowning members, and may be of interest I have brought under your notice a vessel, proved, by her own performance and accounts, to be running at an uneconomical speed, as regards cargo-carrying, on a voyage of considerable length. I have then shown that if the ship had been engaged on a shorter voyage the speed might have been right enough. Then, by comparing the performances of different types, we found that even for this long voyage 12 knots would have been a suitable speed if the vessel had been of finer form. We have seen how largely the



proportions and forms of ships, as well as the speeds, are affected by the length of the voyage. Indeed, in the sailing of existing ships, and in the designing of new ones, the best results can only be obtained by regulating the speed and form by the length of voyage to be undertaken. Comparing what might be called fine and full ships, we have seen how much greater, even for cargo-carrying, is the commercial efficiency of the former than of the latter for any ordinary voyage ; and we come to this, that, if the superiority of very full ships is not altogether more apparent than real, it is at least confined within the narrowest limits, and only exists when the coal supply is an infinitesimal part of the dead weight carried. The question is, I think you will admit, a complex one, and incapable of

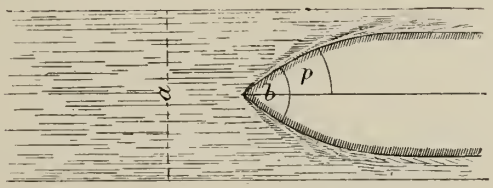
any single solution. The circumstances of each individual case must be studied if the best results are to be got. Instead of the shipowner determining the whole possibilities of the ship by fixing in a haphazard way both the dimensions and the carrying capacity, it would be far more to his interests if he relied on the shipbuilder to do this part of the work, as it takes a naval architect to select proper dimensions and to say what the corresponding dead weight should be for any service; whereas, when this is done, the shipowner might, without much risk, be trusted to make the model himself.

APPENDIX.

Comparisons which are based on the performances of a limited number of vessels are justly open to suspicion, unless it is clear that differences in the power required to propel the vessels are not accidental or due to outside causes, but are really due to the types of the ships.

To allay doubts on this point, I pointed out that the power required to propel each of these vessels was very nearly in proportion to the beam, plus the draught multiplied by the co-efficient of fineness, because I find that most of the trial data that I have been able to collect lead me to believe that this gives an approximate measure of the fulness or fineness of the vessels, and that the power required generally follows this very closely.

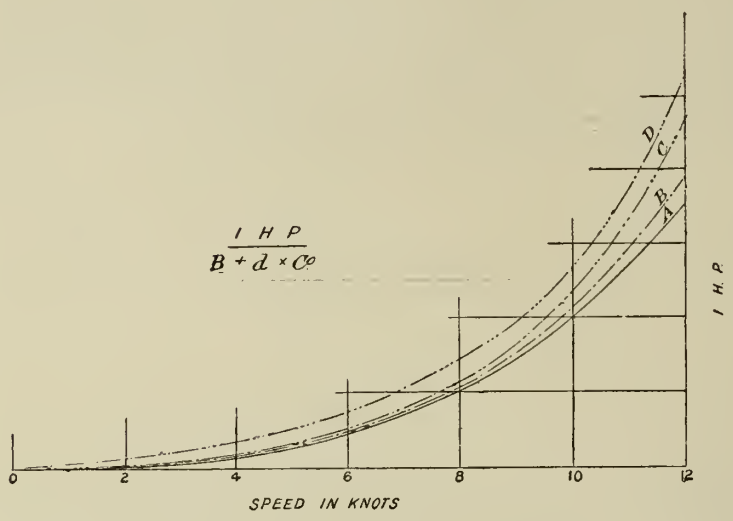
The principle of the thing is, that the resistance of the water to the ship (which is due to something else than skin friction) in its passage past the sides, will be largely in proportion to the number of stream layers of water that are diverted out of their course by the bows of the vessel at A, and also in proportion to the angle of divergence at *b*. (See block figure.)

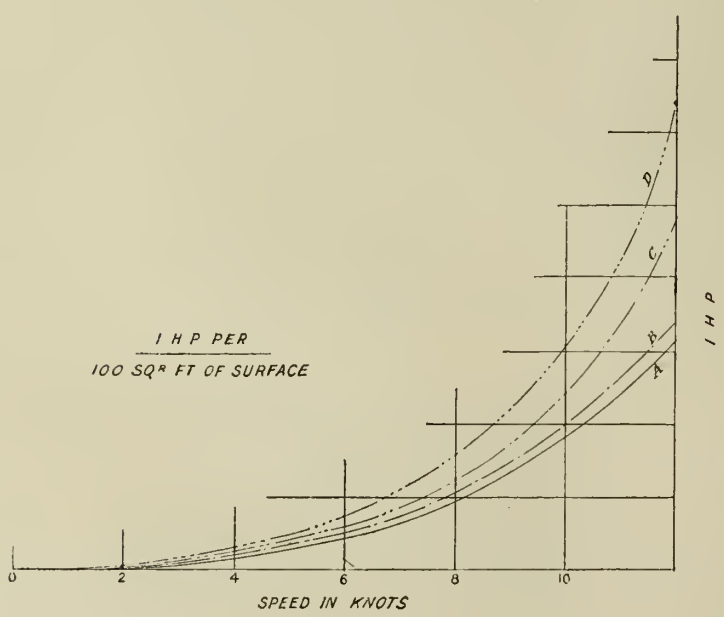


The same may be said of the action of the water which passes under the bottom; or in vessels of the same displacement, the resistance will be largely in proportion to the beam and the draught of water, and to the angle at which the water is diverted from its normal course.

$(B + d) \times$ co-efficient roughly expresses this, and the power is found to correspond generally with this measure much more closely than with wetted surface or either of the Admiralty formulæ that disregard form.

The figures below will illustrate this for the vessels referred to. The Admiralty formulæ, $D^{\frac{2}{3}}$ and mid area, are, for these vessels, little better than surface as a measure of power required.





The differences that Mr. Froude found between the models of the *Merkara* and the three others tried are accounted for when the same method of comparison is applied.

The displacement was in each case 3,980 tons, and at 14 knots, which may be called full speed for vessels of this size and class, the following are the powers required and other particulars :—

	Dimensions and Draught.	Co-efficient of fineness.	I.H.P.	(B + d) × Co-ef.
A	369 × 37·2 × 16·25	·64	2,870	34·17
B	368 × 45·8 × 18	·47	2,670	30·00
C	318 × 49·4 × 19·32	·47	2,620	32·28
D	294 × 45·56 × 17·89	·60	3,220	38·0

The $\frac{\text{I.H.P.}}{(B+d) \times \text{co-ef.}}$ gives 84, 89, 81, and 85, or 9 per cent. between the highest and the lowest.

To show that the performances of the vessels referred to in the paper are not single examples, a few others are added to them in the following table; the displacement in each case being 4,200, and the speed 10 knots.

	Dimensions and Draught.	Co-efficient of fineness.	I.H.P.	(B + d) × Co-ef.
A	328·7 × 35·1 × 20·5	·635	725	35·12
B	309·5 × 38·2 × 20·4	·61	778	35·74
B	307·5 × 34·2 × 20·4	·683	832	37·2
C	286·0 × 35·1 × 19·95	·734	951	40·40
C	286·7 × 36·08 × 19·07	·745	958	41·0
C_2	258·8 × 39·4 × 20·25	·712	989	42·47
D	269·8 × 40·8 × 18·9	·708	1,140	42·27

It will be acknowledged, I think, that (B + d) × co-ef., fairly measures the power, whereas the displacements being the same, the $D^{\frac{2}{3}}$ would be the same for all, and is therefore of no assistance.

The following figures, taken from the speed and power curves of eight steamers, all tried in the same way, will be sufficient to illustrate the statement that vessels having large dimensions, wetted surface, and displacement, are often driven with the same power, provided that their lines are sharp, as smaller vessels, the lines of which are full; and this will show how much form, apart from mere length, has to do in the average cargo steamer with the resistance, and supports the statement that the difference in resistance of the vessels compared in the paper can be accounted for by their forms, and that the performances are therefore not accidental.

	Dimensions.	Displacement	I.H.P.
1	225 × 34 × 11·6	1,709	1,454
2	290 × 33 × 14·75	2,740	1,444
3	230 × 35 × 13·32	2,095	1,613
4	380 × 38 × 15·6	4,210	1,630
5	274·6 × 34 × 14·7	2,338	1,273
6	315 × 33 × 13·9	2,695	1,270
7	225 × 34 × 14·23	1,820	1,480
8	297 × 33 × 13·10	2,462	1,126

DISCUSSION.

Mr. J. H. Biles: My Lord, as Mr. Hamilton was good enough to commence the discussion on my paper yesterday, perhaps therefore he will not think it too great a liberty if I commence to discuss his. I am not intending to discuss it with any adverse object in view, because, if he will allow me to say so, I have nothing but praise for this paper. It is a paper that is the natural outcome of progressive measured mile trials, and it shows what very valuable results may be deduced from these trials, notwithstanding that some people are disposed to think them of very little value. The value of this paper is discounted to some extent by the fact that probably—I am speaking subject to correction—the trials are made at comparatively light draughts. That points to the advantage to be gained by trying ships at deeper draughts, and it is at present one of the weak points in progressive measured mile trials that ships as a general rule are tried at draughts which are too light for ordinary practice. The best method of showing the desirability of carrying out trials at deep draughts is by papers of this kind, which bring the shipowner and shipbuilder into more direct communication, and show the shipowner that his interest and that of the shipbuilder are in many instances one and the same. If shipowners after they had their ships loaded ready to start were to place them for two or three hours at the disposal of the shipbuilder on the measured mile, very valuable information might be deduced, from which data of a similar kind to this could be worked out free from the slight suspicion which this paper has, on account of the results being deduced from trials made at a very light draught. I think that the whole reason of writing this paper is summed up in the last sentence, where Mr. Hamilton probably is expressing what he has felt with a great many others, the difficulty of deciding the question of who shall, or who is best able to, fix the

dimensions of a ship. Shipowners as a general rule have considered that they are best able to do it, but, my Lord, without wishing to be pugnacious at all, I beg to submit, in conjunction with or in support of what Mr. Hamilton has said, that the shipowner is not the person who is best able to determine the dimensions of a ship; the shipowner is the person who is best able to state the facts —who is best able to lay down the conditions that the ship is intended to fulfil, but he should confine himself to stating only the conditions which it is absolutely necessary to fulfil, and not more than are necessary. When he does that, the person then who should best be able to decide the dimensions of that ship is the naval architect. I take rather high ground on that account, because I have had some considerable experience in that direction. The firm which I serve takes a very independent standpoint in connection with that, as they have a tendency to decline to do work unless they have something to say in connection with fixing the dimensions of ships, because, if the shipbuilder builds a ship which is of improper dimensions the discredit attaching to that, or the uneconomical results which follow from that, must ultimately come back to the shipbuilder in the shape of a loss of reputation, and therefore it is a wise policy on the part of shipbuilders and naval architects to take up this ground at once, and say it is not a shipowner's question, but a shipbuilder's. The question treated by Mr. Hamilton is capable of considerable extension, and when it is extended it may be as well to introduce a little correction into it. It may be a refinement, and it may be difficult to make the correction, but I think it may be as well to state it, and that is, that one element of long voyages such as those which are shown at the slow speeds, is that the vessel is longer afloat; when a vessel is longer afloat and she is in tropical waters, her bottom becomes more foul than when she is on short voyages and has the opportunity of docking, and that may introduce an element of resistance which may modify to some extent the results which Mr. Hamilton has brought out so clearly. This may be worth while taking account of, and if shipowners will co-operate with shipbuilders and give some *data* on this question, any further investigations made in this direction may be corrected to that small amount. Then with respect to this question of the fineness of ships, I am sorry that Mr. Hamilton has not put into this paper which he has laid before this Institution the results which he laid before the Institution of Shipbuilders and Engineers in Scotland; because there he gave some proof, or he gave some examples of, the statement which he makes here, that the breadth *plus* the draught multiplied by the co-efficient of fineness is a fair measure of the indicated horse-power of the form. I must say that the results which I have at my disposal do not bear that out very closely. Mr. Hamilton intimated yesterday that he considered 7 or 8 per cent. near enough; certainly the results I have are not nearer than that, and perhaps those who have wider results to draw upon may be able to confirm it. But in a question like this, I think, when a gentleman of Mr. Hamilton's reputation, having the facilities for data of this kind which he has, advances a formula which is very simple and may be very useful, it would be worth while for other gentlemen in connection with this Institution to follow up the same line of investigation, and if possible to corroborate him, or if not to say how much value was to be attached to it. I may have at some future opportunity, or when this paper is printed in the Transactions of the Institution, to put some results here in connection with Mr. Hamilton's paper which bear out this or otherwise. Mr. Hamilton may be good enough at the same time to incorporate with his paper the actual figures which he gave in his Glasgow paper.

Mr. B. Martell: My Lord, the Hon. Isaac Smith, from America, would like to make some remarks on this question.

Mr. Isaac T. Smith: My Lord, I have listened, with much profit and very great pleasure, to the deliberations of the Institution this evening, the first meeting it has been my privilege to attend,

I was particularly impressed with the importance of the subject presented in the last paper, "The *Speed* and *Form of Steamships*," by Mr. Hamilton, and listened with much pleasure to the admirable way in which he illustrated and enforced his opinions. I learn from this paper, and from some observations of my own, that in the model of vessels regard has to be paid, not only to the character of the lading, but also to the degree of speed at which it is intended she should be driven, which last consideration brings us to the subject of fuel—the quantity of coal that must be taken to prosecute the voyage within a given time. Great speed means a great consumption of coal. This is the real serious concern in the management of steamship property. Mr. Hamilton tersely says: "There is nothing, I imagine, that gives the steamship manager more care and thought than the matter of coal supply, and there is nothing the naval architect has to deal with in designing ships that asserts its importance more than the *fuel question*. If coal only weighed half what it does, what possibilities there would be in steam navigation! It is possible that a refining process may some day be discovered, and the weight of coal reduced." Now, when it is considered that, according to established tables, only one-tenth of coal is utilised in making steam, and the residue goes to waste, it is a very severe burden to have so much space occupied, and so much weight carried, to so little profit. It has long engaged the attention of thoughtful persons, how it may be possible to reduce fuel to a smaller compass; to consume the essence, so to speak, separated from the other part; to get the *gases*, for that is all that is derived from fuel—any fuel that really enters into combustion and gives out heat. It will doubtless be interesting to this convention to learn that an ingenious American, Dr. Charles Holland, has, by a simple inexpensive apparatus, consisting of an iron retort not larger than the paper shade on the lamp in front of the President, decomposed water by the use of gas from naphtha, and brought the oxygen and hydrogen of water into this source, and to know that it is working with perfect success: that a locomotive using these gases alone is successfully running, conveying passenger and freight trains with perfect regularity in the United States; and it is also used for domestic and mechanical purposes. The difference to a ship between this, as compared with the present fuel coal, will be, say—

1,000 tons coal weigh	1,000 tons
20,000 gallons of naphtha, a cheap product of petroleum, used with the water, and equal in power to 1,000 tons coal, weigh	70 tons
Saving in weight	930 tons
1,000 tons coal measures over	1,000 tons
20,000 gallons naphtha measures less than	80 tons
Saving in space	920 tons

So that I may say, that, using the gases for fuel, there will be a saving of good nine-tenths of the weight and space of coal.

Mr. B. Martell: My Lord, I am very pleased to see with us here such celebrated representatives of the North-East Coast as Messrs. Wigham Richardson and Mr. Raylton Dixon, who have built these cargo-carrying steamers that have been admitted to be by the Clyde builders, as Mr. W. Denny has stated here, so admirably suited for dead-weight carrying. They have really perfected those steamers in a manner that leaves scarcely anything to be improved with regard to their economical uses. The Clyde shipbuilders, where formerly they built ships of fine form with great rise of floor, have frankly admitted, much to their honour, that they have taken a lesson from the North-East Coast builders, and they have improved their models for some purposes of trade to a great extent, so that

they are admitted to be much more profitable for general cargo-carrying purposes than the form of vessels they originally built in that river. Now, it would be very interesting to hear the opinions of Mr. Wigham Richardson and Mr. Raylton Dixon on this very important question which has been brought forward by Mr. Hamilton. No doubt there is a combination required with reference to the length of voyage and the forms of ships, and when we know from our experience of the North-East country owners, who build ships of the extreme box form with very low speed comparatively, vessels of 9 knots to 10 knots at the outside, and are dividing their 15 per cent. dividends, and we see that other owners of steamships which are realising their 12 knots and scarcely paying anything, it seems that there is some information we require, to know how it is that owners of ships of a different form altogether from what Mr. Hamilton has set forth here can realise these large profits as compared with the ships which are the ideal ships represented by him. At the same time, I think that we are very much indebted to Mr. Hamilton for taking the trouble to write this paper, and to bring before us all this information, and it is an illustration of the value of this Institution, because in former times we had no means of getting such information as this and making comparisons. It was what was formerly placed on the doors of large firms: "No admittance except on business." Now we find that there is very little craftism in this country, that places are open, and we are free and open to show what we can do and invite competition. So it is with reference to these papers. A paper bringing forward these many questions of forms and speed of vessels as well as profits and such like, shows as much as anything the great value of this Institution in being able to place before us such questions, so that we can discuss them, and on the basis of that see what we are doing with reference to great questions like this. Therefore I hope that our friends here who are so competent to speak on this subject will favour us with their opinion on it.

Mr. J. Inglis: I think Mr. Martell has somewhat misapprehended the intention of this paper. I don't think Mr. Hamilton proposed to call attention to the superiority of one class of ship built in one district over another ship built in another district, but rather to indicate the manner in which comparisons might be made by any shipowner or any shipbuilder to arrive at the best type possible for a particular trade. I hope that this paper will have the circulation among shipowners that it merits, because the question is a very important one for them. I think it might suggest to them that it is scarcely worth while to reproach the Board of Trade for throwing away half a million a year, as we were told yesterday they did by mal-administration in the matter of boilers, when they find that the shipowners of this country must be throwing away sums annually, by their own ignorance, compared with which half a million pounds is a very small sum. It would be advisable that they should place themselves in closer relation to the shipbuilders they employ, in order to get the best thing possible for their purpose. According to the general custom followed in obtaining tenders for a new vessel, the practice is to print the specification with every detail included in it, and to invite tenders and to accept, as a rule, the lowest offer. I leave you to judge how, by that mode of procedure, one is calculated to arrive at the results which Mr. Hamilton indicates are to be aimed at. It is well known to those who know the great shipowning companies of this country, that those companies which have been most successful are not those who have sent their specifications broadcast over the country, but those that have been in intimate relation with a builder in whom they had confidence. It would be invidious to mention names, but anyone who knows shipowning and shipbuilding can easily pitch upon the names for himself. With regard to the excellence which our friends on the East Coast have attained in the building of cargo-carrying vessels, no one would be more ready to admit it than those of the Clyde builders who know what East Coast ships are; but I think that arises a good deal from the fact that the

Clyde builders are so much engaged in building fast vessels for the carriage of mails and passengers, that they are content to leave the cargo-carrying ships to the East Coast.

Mr. MacFarlane Gray: Some years ago I gave my attention to this question in a very elementary fashion, and I am very pleased indeed to have had this opportunity of having this paper laid before us, because it contains a great deal of practical information which was not available to me at the time. I then published a very simple Rule, one as to the saving of coal by lengthening the voyage, or the cost in coal by shortening the voyage; it was to multiply the consumption at one speed by the other speed, to take the difference of the two and divide by the difference of speeds, and that gives you the cost of saving time upon the voyage in tons per day. To take a general illustration, where the resistance to a vessel increases as the square of the speed, we have also this result, that the rate of cost of saving time is just double the rate of consumption. That is, if you are burning 20 tons of coal per day, the value of saving time ought to be 40 tons of coal a day, looking only at the coal saving. It is a very simple matter for owners to say, This steamer is burning 24 tons of coal per day; is she making 48 tons per day for me? If she is not, go slower. When additional cargo is carried for the fuel saved, the freight rate has to be added to the price of the coal; then, if twice the consumption per day, multiplied by this coal-freight rate, be *less* than the sum of wages and returns per day, the speed may be increased with profit; if it be *more*, go slower. This can be very well illustrated by the examples given in detail in the paper. The coal is 12s. 5d. per ton, but the oil is in this sense equally fuel—it is 12 per cent. of the coal—say, fuel, 13s. 10d. per ton, or £·69. At eight knots the consumption was 13 tons; twice this is 26.

Freight rate ...	£3·25
Fuel rate	·69
	3·94 × 26 = £102
Compare this with £43,706 ÷ 360 = £121	

The speed could therefore be slightly increased with profit, because 102 is less than 121. At the ten-knot speed the comparison comes out £204 and £115, showing that this speed is too high. At the twelve-knot speed the figures are 343 and 105—still further wrong. The meaning of these numbers is, that at twelve knots £343 is paid away to get £105. The amounts £121, £115, £105, may be called the actual daily demurrage, which ought to be just equal to the freight and cost of two days' fuel. The intended demurrage rate may be fixed commercially for any vessel, and then that divided by the freight and cost of two tons of fuel gives what ought to be the number of tons of fuel consumed per day with only terminal coaling. Mr. Hamilton's graphic method is much better than this, but perhaps this rule of thumb will be applied more readily.

Mr. Geo. Renwick: It is getting rather late, but as a shipowner, perhaps Mr. Hamilton will not object if I ask him a question. As naval architects must acknowledge, it is to shipowners they have to a great extent to look for orders, and I dare say you would be glad of any suggestion from a shipowning point of view. Mr. Hamilton says, "How much better it would pay if ships were always built of suitable proportions for the length of voyage or service on which they are to be engaged." I would ask if Mr. Hamilton could give any advice as to this point: What would he advise an ordinary merchant shipowner to do who is building a ship, say, of 4,000 tons burthen? Which ship may be one voyage going to the Black Sea, if freights were good there; the very next voyage she may have to go to New Orleans or New York, and if freights fall in America, she is sent off to China or Calcutta. Now those distances vary to a very great degree indeed, yet the

ship has the identical engine, and is burning identically the same quantity of fuel on each voyage. I have not time to go fully into this question, but I want to know if Mr. Hamilton would recommend a shipowner to put an engine in the ship capable of developing 2,000 indicated horse-power, which he might use when he was going a long voyage, and only to develop, say, half of that power in going a shorter voyage? I think if we have to put in a very much more expensive engine, we lose by the weight of the engine and the extra boiler space, and by the extra room taken up by the engines and boilers. As I said before, I have not had time to go into this paper, but I think there is a mistake somewhere, because I know, for a fact, there are many ships built on the North-East Coast which are sometimes going to the Black Sea, sometimes to Calcutta, and sometimes to America, which vessels can compete with any ship ever built going perhaps twice the speed or half as much again. I should be very glad if Mr. Hamilton could give me some suggestion as to what would be the best form of ship, and what is the best powered engine to put into such a ship as I have mentioned.

Mr. J. MACGREGOR: I think this is a question that ought to be discussed in connection with the present paper. We have heard a great deal just now of the necessity of shipowners consulting naval architects as to the requisite proportions of the ships they require. That, I think, must be universally acknowledged, but a shipowner, as a rule, places himself at once in the hands of a contractor. If a person requires a railway, or docks, or a church, or anything of that kind, he goes to an architect or a civil engineer, and asks his advice, and gets a scheme properly matured and deliberated upon; specifications and plans are drawn out, and are put into the hands of contractors who tender for them, and the consequence is that every contractor tenders for exactly the same thing: but, on the other hand, if any person requires a ship he appears to think that all that is necessary to do is to go to the shipbuilder at once, who then acts in the double capacity of naval architect and contractor, and this is to a great extent the cause of some of the peculiarities in ship construction that we see.

Mr. J. HAMILTON, Jun.: My Lord, perhaps before I begin to reply you will allow me to express the pleasure I feel that my paper has been criticised by a distinguished American. I had no idea that we had got so far that the same heat could be got from fuel taking up only a tenth of the space of coal. If we could even get the same heat out of half the space I think it would be a great step, and I shall be very much interested to hear later on how the Americans, who are so proverbially ingenious, managed to do it. Mr. Biles referred to the comparatively light draughts at which the trials referred to in my paper were made. We have had trials of steamers made at loaded draughts; it is becoming more and more the custom to try them at loaded draughts, and the results are certainly more valuable when the vessels are tried in that way; but these vessels were all tried at similar draughts, with the propellers well covered. It would be a very poor compliment to this Institution if I were to come here and base all this superstructure upon a bad foundation—at any rate, unless I believed I had a very good foundation to base it upon. Of course, I have only given a few examples, but I can support those, and I shall be very glad to do so, in the Transactions, if you will allow it, by many examples. I shall only give you one, as time presses, but I said yesterday that the measure of a ship's form I use to arrive at the power agrees with Mr. Froude's four model experiments. I need not refer again to that, but in Mr. White's valuable book he gives the model experiments of the *Greyhound* and the *Merkara*, the *Greyhound* with a displacement of 1,160, and the *Merkara* with a displacement of 3,980. The displacement of the *Greyhound* was less than a third of the displacement of the *Merkara*, and yet the resistance even at such a slow speed as four knots was $\frac{6}{10}$ths of that of the *Merkara*, at twelve knots their resistances were exactly

the same. I merely state this to show that here is a ship of the size of the *Merkara* going at twelve knots, with the same resistance as the *Greyhound*, which had a displacement of less than a third. The conclusion I draw from that is, that the difference is due to difference in form, the ratio of length to beam in one being 9·7, whereas in the other it was only 4·8. I have here also a list of vessels tried on the measured mile, where no corrections are made at all, showing large vessels with great length and beam running with the same indicated horse-power, but having a great deal more displacement than shorter, fuller vessels. However, I shall not take up further time with that. I think I may say that I have established the basis on which my figures are made, but of course it is a very difficult subject, and there is not altogether a very large amount of data to be got even in the whole country; I believe, however, I have got a very large amount of it that bears me out. I do not imagine that either of the corrections I have made can possibly make any very large difference. The one correction was using the late Mr. Froude's law of comparison to adjust slight differences in length to bring the ships all to the same displacement. Now, if Mr. Froude's law is good for passing from the model of the ship to the actual ship, it is surely good for differences of 25, 30, or 40 ft. of length when the ships are exactly similar ships; therefore, I think, there can be no great discrepancy there. Then in the matter of displacement, the slight differences in the displacement are corrected by the Admiralty formula. I think some of us are in the habit of using that daily, and Mr. Biles himself, in his paper, seems to base his method of comparison on that, and, therefore, I think there can be no great discrepancy in estimating the power corresponding to a few feet of extra draught. The same correction being made in each case, the comparison should not suffer. Mr. Martell has referred to the ships of box form, and to the East Coast shipowners dividing 15 per cent. Well, I cannot explain how they divide better dividends than other shipowners. I do not think it can be because the forms of the vessels tend to economise coal, and it certainly cannot be a paying business to run at slow speeds. Mr. Renwick asked me a question with reference to a ship running first upon one voyage and then upon another, and he asked me what I would advise him to do. Well, if the ship is built, if it is a ship in existence, very little can be done; but I would recommend him to study that diagram, and if the voyage is a very long one to slow down the speed.

Mr. Renwick: No, I asked what sort of a ship it would be advisable to build for the trades I named.

Mr. Hamilton: I would recommend Mr. Renwick to build a ship of moderately fine form, because, although I have referred to the gain that is to be got by reducing the speed on long voyages, I do not by any means approve of such a thing—I distinctly disapprove of it. The reduction in speed is got at a tremendous sacrifice, but what I want to go in for is building a ship that is able to run at a small consumption without sacrificing the speed, and, therefore, I would advise Mr. Renwick to go in for a ship of good fair average form, rather on the sharp side.

Mr. Renwick: It was with regard to the power I wanted to get the information.

Mr. Hamilton: The horse-power?

Mr. Renwick: In regard to the speed of the ship.

Mr. Hamilton: I don't follow Mr. Renwick.

The PRESIDENT: This discussion is not quite in order. I am afraid I must stop you, because it would extend our discussions too far.

Mr. RENWICK: Mr. Hamilton has not answered my question.

Mr. HAMILTON: I would terminate my remarks by saying to Mr. Macgregor that if shipowners would go to a naval architect instead of coming to the builder it would very often be a great blessing.

The PRESIDENT: Time is very valuable, and I am sure you will allow me to tender our thanks to Mr. Hamilton for his paper; and I venture to add that of all the interesting papers that have been read, and they have been very many, when this paper is circulated it will be as extensively read in this country as any paper that has been presented to us this year, because it is a paper which essentially affects the pocket, and in business countries the pocket is a very great consideration. But there is one feature of the debate, by no means perhaps the least interesting, which you will forgive me for alluding to, and that is the presence and the part taken by our distinguished friend the Honourable Mr. Smith from the other side of the water. We are always rejoiced to meet our foreign friends and to hear the result of their experience, and I am sure you will allow me to express your thanks, as I do my own, to Mr. Smith for his attendance here to-night, and for the valuable addition which he has made to the discussion.

FOG SIGNALLING.

By J. MacFarlane Gray, Esq., Member of Council.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The probability of collisions between steamers navigating a given area of water increases in the duplicate ratio of the number of the steamers. That is to say, with ten times the number of steamers there would be one hundred times the number of collisions between steamers, other conditions being equal.

The number of steamers is rapidly increasing, and the newspapers keep on telling us of the increasing frequency of collisions, and the crying need there is of some international code of signals by which a vessel in a fog could communicate to vessels in its proximity, more accurately than is now done, the direction in which it is moving.

I have not any complete system of fog signalling to lay before you, but I know that there are many now engaged upon this problem, and I have some remarks to make on details, which I think of importance, to be attended to in any such system; and by bringing these before this meeting a discussion may be provoked, bringing out other important points and an expression of opinion as to the propriety of further international legislation.

There are several signalling machines before the public giving the blasts for the signals automatically. When the blasts are to be given by a steam whistle, it is practicable to make a simple arrangement to accomplish this, as in Barker's Machine, at a moderate cost. If a simple code be adopted, it might be practicable to operate the whistle by hand, and, there being no extra expense incurred, the objection of expense would be got over. After a code has been internationally sanctioned, there would, no doubt, soon be simple mechanical arrangements introduced for signalling automatically, but the question of adopting these or working the whistle by hand might be left to the owner's discretion.

In the system of fog signals at present in operation, the signals consist of one, two, or three blasts. A code in which each signal would consist of four blasts would not interfere with the present system, and it would, therefore, be practicable to add such a code of course signals to the present system, leaving it optional either to signal the course or not.

In any case, it would be well to have every signal in a course code to consist of always the same number of blasts, so that there would be certainty as to whether the whole signal had been heard. A three-blast code is sufficient to divide the compass card into eight arcs, a four-blast code would divide it into sixteen arcs.

If the signals consist of the same number of blasts or notes, there must be at least two kinds of blasts, say, short and long, or high pitch and low pitch, or continuous and intermittent, that is, a clear note and a trill. All these variations have been exhibited by different inventors. A very suitable high and low pitch whistle for steamers was exhibited at the late Exhibition of Marine Engineering by Messrs. Smith & Sons, Nottingham.

The following code of four-blast signals can be taught in a few minutes to anyone who can box the compass. It has only to be understood, it is not necessary or even advisable to commit it to memory. I will describe the signals as for a clear note and a trill, which I will call U and R. For clear and trill, may be substituted short and long, or high-pitch and low-pitch; the sounds U and R being the same as the words "you" and "are," the signals are easily spoken with these names for the blasts.

Any eighth part of a given line can be indicated by the word left or right, three times; thus, left right left would mean of the *left* half take the *right* half, and of that again take the *left* half. That would be the third eighth part from the left. In the same way, right left left would be the fifth, and right right left the seventh, from the left. If, now, we had two such lines, we could indicate any of the sixteen parts by prefixing one word to tell which of the two lines the part is in. For the two straight lines we may substitute two curves, say the two semi-circles of the compass-card, and for right and left we have east and west; to distinguish which of the two semi-circles the part is in we have to say north or south. We may now put in R and U for these. The first blast would always be read to mean north or south, and all the other blasts will be read east, or else west. As there is an R in north and a U in south, say R for north and U for south. East is on the right of a map, say R for east; and as there is a U (or rather two) in west, say U for west.

Let the signal given be R U U R. The first blast R denotes the northern semi-circle. The second blast U denotes the quadrant forming the western half of that semi-circle. The third blast U denotes the four points forming the western half of that quadrant. The fourth blast R defines the two points forming the eastern half of the preceding four points, or the N W b W two points. The R U U R has evidently been read north west west east. When the principle of successively halving an already indicated arc and taking that half which is denoted by the blast is once comprehended, the mind itself provides a compass circle diagram, which it divides, as indicated, at each blast, with the mind's eye fixed upon only the part indicated, without naming the parts. The compass circle having been divided into sixteen parts, through the cardinal points, the middle of each division is a course with "by" in it on the compass card.

CODE.

R U U U	W b N	U R R R	E b S
R U U R	N W b W	U R R U	S E b E
R U R U	N W b N	U R U R	S E b S
R U R R	N b W	U R U U	S b E
R R U U	N b E	U U R R	S b W
R R U R	N E b N	U U R U	S W b S
R R R U	N E b E	U U U R	S W b W
R R R R	E b N	U U U U	W b S

There are optional signals of one, two, or three short blasts now in use, whereby a steamship under way is permitted to indicate to any other ship which she has in sight, that she is taking a certain course authorised or required by the regulations. The only sound signal authorised for a steamer in a fog, when not having another vessel in sight, is a prolonged blast of the steam whistle at intervals of not more than two minutes. There is therefore really nothing in the regulations now to forbid or prevent the use of such a code as that I have described, but as it would lead to great confusion if different companies arbitrarily adopted different codes, an international sanction ought first to be obtained for some code—the one I have described, or a better one.

There would be great difficulty in getting an international law *compelling* course signals, but there would be less difficulty in obtaining a sanction that certain signals when used would have a certain meaning. The same clause as is now appended to Article 19 could be applied to the code adopted. "The use of these signals is optional; but if they are used, the course of the ship must be in accordance with the signal made." The use of course signals need not be confined to fog; it would at other times often be an assistance to navigation to exchange an intimation of courses between meeting steamers.

In addition to course signals, there are other intimations which it would often be desirable to communicate by blasts of the steam whistle. If twenty-eight standard messages be a sufficient number for such signals of danger or urgency, they could be conveyed by a double signal from a three-blast code, always a pair of *different* signals, and signifying the same, independently of which is given first. The repetition of the two signals could not then create doubt, whichever is given first or last.

Although the course code of four blasts does not really clash with the present one, viz., one, two, and three blasts now prescribed as fog signals to intimate how a vessel has the wind, I do not recommend its adoption by sailing vessels, but those in charge of sailing vessels would have also to understand the code if it were adopted in steamers. Although steamers by law have to keep clear of sailing vessels, there is no doubt sailing vessels would be kept more out of the way of steamers if the course of the steam vessel were always clearly intimated to those in charge of the sailing vessel.

To facilitate the putting together of the two courses, I have constructed a dial carrying

two model hulls. One of these, in the centre of the dial, always pointing ahead, represents the vessel A, on board of which it is. The other, carried at the circumference of the dial, represents the other vessel B. The dial is a dumb compass, and it is kept always set for the course A is on. When a course signal is received a handle is turned to that course, the plate carrying the other model is then turned in the direction in which the sound is heard, and the two hulls then constitute a collision diagram of the case to be dealt with.

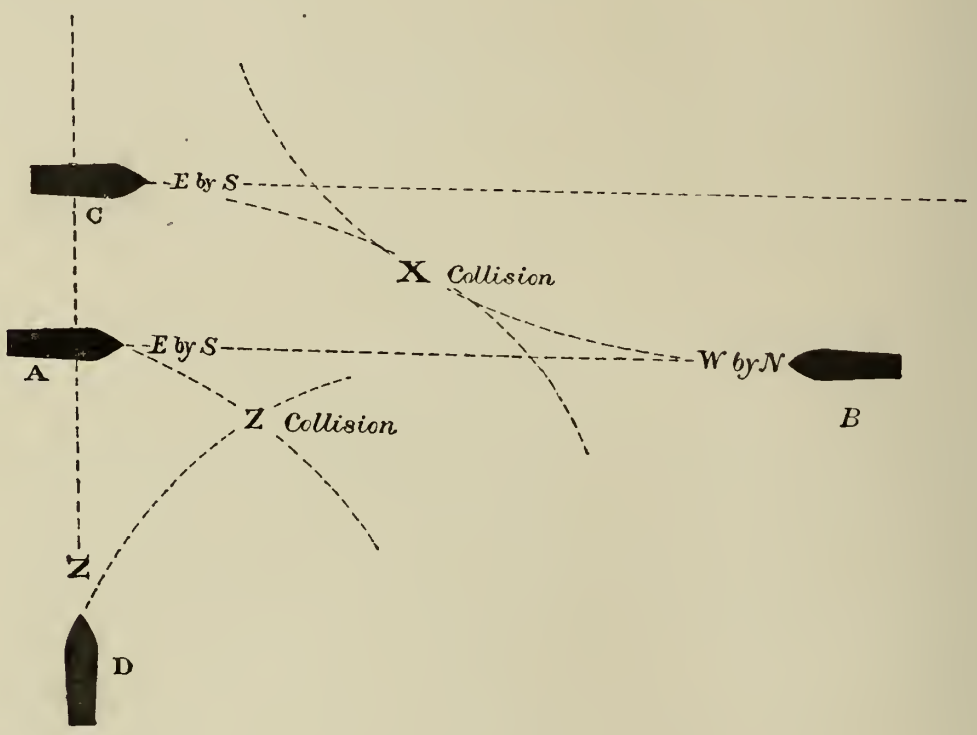
This mechanical contrivance is probably unnecessary, but it would, no doubt, be a help to the navigator who has rapidly to translate the received signals from a compass course to a direction in relation to his own vessel, often under exciting conditions.

I do not read this as a Board of Trade Officer, and what I have said has no weight whatever as indicating what might or might not be approved officially.

DISCUSSION.

Mr. H. Liggins: My Lord, I should like, if you will permit me, to say a word or two upon this subject, because in this Hall, although not before the Institution of Naval Architects, we have had several papers read within the last few years on similar subjects. If you would kindly allow me to make a little diagram I will ask Mr. MacFarlane Gray a question. [See page 284.] Gentlemen, it may be known to many of you that a ship going up the English Channel in the mid-Channel course, and a ship going down the mid-Channel course, the one will steer east and by south, and the other west and by north. Mr. MacFarlane Gray has told you how those circumstances may be indicated to two ships. Now A is the ship going east and by south up the English Channel, and B is the vessel going west and by north down the English Channel. If A and B are right ahead of each other and keep their course, collision is inevitable, but if A happens to be nearer the land as at C, and both keep their course, they will clear each other; but as neither of them can know exactly where each other is, and both port, a collision may happen at X. Do I make myself understood? I mean to say it depends very much upon where the ship is when she signals the course that she is steering. Another example: suppose A and D understand by signal that one is steering east by south and the other north, and being in the dark as to actual position, both port, a collision is likely to happen at *Z*. But I should ask, is it allowable by the Board of Trade Rules to run at a speed which may bring you into danger in a fog? the rule being that all vessels in a fog, &c., shall go at a moderate speed. But a case which startled us very much the other day happened at Liverpool. The Inman steamer *City of Brussels* had arrived from New York in the Mersey in a fog, and she turned her head round towards New York, and was really at rest when she was run into by the steamer *Kirby Hall*, who ought to have heard her continuous blasts of whistle or bell. The case of the *Cimbria* the other day was somewhat similar. I believe she was going as dead slow as she could well be, when a vessel crossing at right angles to her came into collision with her, and sank her. As far as my experience goes—and I have watched every case for the last forty years I have seen in the papers—it is the neglect

of listening to signals, and taking no notice whatever of them, which causes the destruction of ships. I know by experience in the English Channel, in a fog, it is exceedingly difficult to understand fog signals, because you cannot tell the direction from whence they come, and you hear vessels all around you. I was once in the English Channel and counted 700 vessels, and that was in fine weather; but, supposing it had been in a fog, we should have had 700 vessels making fog signals at one time. It is a very common thing to see six or eight or ten near each other in the Straits of Dover, and often I have seen the whole Downs fleet of 500 or 600 vessels get under



weigh in a morning after a long spell of south-west gales. They are as thick as can be in the Straits of Dover, and for many miles down Channel, and they are very often caught in fogs. I do not think any new signal is required, for no lights are visible in a fog, and I do not think, from the deadness of sound which is always incident to a fog, they can be of much value in preventing collisions. The only preventive of collision will be to stop, and that is the Board of Trade Rule as it now exists. Besides, the international element of this difficult question rather points to the retention of the Board of Trade signals as they now exist. Stop is a safe and easy signal.

Mr. MacFarlane Gray: It must not be supposed that I wish this system to be adopted at all.

Mr. Liggins: No; I did not suppose that you did.

Mr. MacFarlane Gray: I did not give my reason for producing the paper, which is, that we have so many systems brought before us that I thought it right to show that we can make codes ourselves when wanted. I think in this case you know better than I do, Mr.

Liggins; but if the vessels both signalled the same course, they would both have to port—the law says so.*

Mr. LIGGINS: They would both go clear if they were in the position of C and B.

Mr. MACFARLANE GRAY: But that is not enough; they should be both bound to port.

Mr. LIGGINS: That might bring them into collision at the point X, and in the other example at *Z*.

The PRESIDENT: Gentlemen, I am sure you will allow me to thank Mr. MacFarlane Gray for this paper. I quite believe it is an earnest and honest endeavour to deal with an enormous difficulty, but whether it is quite overcome yet, of course remains to be seen. I am sure you will all let me thank Mr. Gray.

* In the paper I have said, "I do not recommend its adoption by sailing vessels." I am dealing with steamers only, and it is the law for steamers I quote here—steamers approaching in the same line in opposite directions. J. M'F. G.

ON A METHOD OF OBTAINING THE DESIRED DISPLACEMENT IN DESIGNING SHIPS.

By R. Zimmermann, Esq., Member.

[Read at the Twenty-fourth Session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth in the chair.]

I have been induced to prepare the present paper because at former meetings of this Institution any paper treating on the ordinary drawing office work has been favourably received by the members. My experience is not very large, but I believe what I am going to lay before you will prove of some value in the drawing office, and as a new member I consider it my duty to make the Institution participate in whatever little I may have been able to achieve.

Up to a comparatively recent date, a model was preferred in most private shipbuilding yards to a design of a ship on paper, and at the present day there are, I understand, even on the Clyde, that stronghold of modern naval architecture, shipbuilding firms of repute with whom the art of designing a vessel on paper is either not known, or else not sufficiently appreciated to be the practice. A model shaped to the taste of the shipbuilder was good enough at times when even the calculation of a displacement was unknown; but when carrying capacity and speed form such important factors the question of obtaining a certain amount of displacement becomes paramount. Now, everybody will agree with me if I say it requires an extraordinary amount of skill and experience on the part of the model-maker to cut a model for a certain displacement or for a certain tonnage, and the process of calculating the displacement or the tonnage from a model takes more time, and gives a less correct result, than similar calculations made from a sheer-draught, and if the model be found too fine an entirely new one has to be made; while fining a model also causes much more work than either fining or filling out the lines of a sheer-draught when the desired displacement has not been obtained.

Unless a sheer-draught of a similar vessel of nearly the same dimensions be at hand, the design of a new vessel is generally done at hazard, no previous calculations being made to ensure at once obtaining the required displacement, and in such cases I have seen considerable discrepancies occur, though of course draughtsmen with much experience in designing generally come very near the mark—at any rate, nearer than one would expect the model-maker to come. In the present paper I wish to lay before you a method by which, with very little preliminary calculation, a very close approach to the correct displacement may be ensured, and in which the error will always be so small that keeping

the offsets for the loft and for the model full or fine, as the case may be, will be sufficient to rectify it without necessitating an alteration of the lines, whereby much valuable time will be saved.

At the "Gewerbe Academie" in Berlin, I was taught a method of designing which always ensured the correct displacement, but which required a great amount of figuring during the process of designing, and therefore greatly hampered the progress of the work, as well as the freedom of the draughtsman to shape the lines to his taste and to fair the ship. It was by means of a displacement curve—not displacement scale—the ordinates of which represent on a certain scale the areas of the corresponding cross-sections of the vessel. The areas of the cross-sections in the design had to be continually calculated, and to be kept equal to the areas indicated by the displacement curve, and the required displacement was necessarily obtained. My method of designing is similar to the one just mentioned in the first stage only, commencing with a line of a shape something like a displacement curve, but doing away with all calculation during the process of designing.

All naval architects who have some practice in designing ships know that lines which appear as diagonals in the body plan, if they are expanded in the half-breadth plan, greatly help to fair the vessel—much more so than the other lines usually used to fix the shape of the vessel. I had a good deal of designing to do at one time, and very soon learnt to appreciate the value of a diagonal drawn in the body plan from the centre line at the height of the load line to the point where the rise of floor line intersects the side line; and I noticed that the spots for cross sections on this line, in conjunction with the spots on the gunwale line, practically settle the shape of the cross sections, and consequently the shape and displacement of the ship. I then made this line, expanded in the half-breadth plan, the basis of operations—given, of course, outlines of midship section, stem, stern, sheer, and gunwale—by proportioning it off from other similar ships, and I very seldom got far wrong. In comparing ships, we used at the Gewerbe Academie a co-efficient called the "midship-section cylinder co-efficient," which is, the displacement in cubic feet divided by the product of area of midship section and length. This was a sort of forerunner to "Kirk's Analysis," giving a better idea of the fineness of the ends than the ordinary displacement co-efficient does.

When I began to analyse my "bilge diagonal," I found its co-efficient of fineness to come very near to the midship-section cylinder co-efficient of the same vessel. I then calculated all co-efficients for a number of vessels, and found that the bilge diagonal co-efficient bears a certain relation to the midship-section cylinder co-efficient; being about equal to it in very full vessels, and getting gradually larger than the midship-section cylinder co-efficient as the ships get finer, though not in the same ratio.

On Fig. 1 (Plate XX.) the abscissæ are midship-section cylinder co-efficients, and the ordinates are bilge diagonal co-efficients, giving a number of spots from vessels actually built, and the black line is a curve passing through some of these spots and lying very near to the

others. The two end spots of this curve are positively correct, for if the vessel is a square box both the midship-section cylinder co-efficient and the bilge diagonal co-efficient are 1·00, and if the vessel below water is a pyramid or a half-cone (the other extreme case) the midship-section cylinder co-efficient will be ·3333, and the bilge diagonal co-efficient ·5. Those spots which do not actually lie on the curve are not far enough away—the scale being very large—to materially affect the result. If in either of these cases the bilge diagonal had been constructed to a co-efficient taken from the curve, its area would have differed so little from the actual one that the displacement would have been affected to the extent of only a few tons. All the vessels which are used in constructing this curve were of ordinary shape, though of different dimensions, proportions, and co-efficients, as the accompanying table shows, and I do not doubt a moment that other people who build vessels of unusual shape—for instance, with the end frames full below and very lean at the load line—will find that co-efficients calculated from their lines will give another curve, flatter or rounder as the case may be, but having the same end-points as the curve given in Fig. 1. The following remarks, therefore, refer for such people to a curve which they must construct for themselves from their own data.

This bilge diagonal diagram now is the means of finding the co-efficient of the area enclosed by the bilge diagonal for a proposed vessel, length, breadth, draught, displacement, and midship section being given, or having been fixed by the designer. The latter must calculate the midship-section cylinder co-efficient, set it off as an abscissa on the diagram, square the point up, and measure the ordinate to where it intersects the curve. The length thus found gives him the co-efficient of fineness for the area of the bilge diagonal, upon which he must base his design, to obtain the required displacement. The design has been prepared by the drawing in of all outlines and straight lines, and the next thing would then be to draw a curve enclosing an area of the bilge-diagonal co-efficient, and whose length is the length of the load line, and whose greatest ordinate at the middle is the length of the bilge diagonal on the midship section. This is most readily done by means of a parabola, viz., if—

A = Area of the surface enclosed by the bilge diagonal.
B = Length of bilge diagonal on the midship section.
L = Length of load line.
n = Co-efficient of the parabola, enclosing an area equal to that enclosed by the bilge diagonal.
y = Ordinates of the parabola.
x = Abscissæ on each side of the centre line.

Then we have—

$$n = \frac{A}{LB - A}$$

And—

$$y = L\left(\frac{x}{B}\right)^n$$

I have prepared a diagram (Fig. 2, Plate XX.), from which the offsets y or $B - y$ for six stations on each side at equal distances from the centre line can be proportioned off to

suit the length of B, giving a parabola with an area of the required co-efficient The diagram (Fig. 2) is constructed for areas with co-efficients of from ·6 to ·85, and all that it is necessary to do is to square a line up at the station coinciding with the co-efficient of the required bilge diagonal, and to proportion the six spots obtained by the intersection of the perpendicular line with the curves of the diagram, so that the height of the sixth spot becomes equal to the midship breadth of the bilge diagonal. These spots, set off in the half-breadth plan at the six stations on each side of the midship section, give a parabola, which will have to be altered a little, without of course altering the area, generally in the manner shown on Fig. 3, the dotted lines representing the parabolas, and the full lines the bilge diagonals. This altered line is then the bilge diagonal necessary to obtain the desired displacement, and spots from it are transferred to the diagonal in the body plan, and the first two or three cross sections nearest the midship section can generally be at once drawn in for good, and one or two of the others may be sketched in, the spots on the gunwale line serving as a guide. The load line and outside buttock line are then put in, and when these are fair with one another, other buttock and water-lines are drawn, and the ship is faired in the usual manner, always taking care not to alter the bilge diagonal in such a manner as to affect its fairness or area, but no further calculation is needed until the vessel is faired, when the displacement will have to be calculated in the ordinary way.

Independently of the use I have made of this bilge diagonal to obtain the required displacement, its value for fairing the ship is very great, and yet this fact seems to have been lost sight of by many people, and I remember an instance, when two or three of the aftermost frames in a small vessel, designed by competent authorities in naval architecture, had to be cut out, and they were considerably fined in order to allow of the fitting of the plates under the counter. The design had been accurately copied, and laid down in the loft, but its fairness having never been doubted, the bilge diagonal, without which it had evidently been designed, had not been run. When this line was afterwards laid down, it showed a very large lump aft, and in the other ships of the same class this lump was taken off, and the line was filled out between the after end and the midship section, where it appeared flat, so as to keep the same displacement.

In a case where an old design has to be fined or filled out, the quickest way of again getting fair lines with the correct displacement, is to fine or fill out the bilge diagonal, to carry the spots over to the body plan, and to draw through these spots cross sections, which resemble the old ones in shape.

Most of the gentlemen present with great practice in designing will hardly require to use the method I have endeavoured to explain to you, but I am confident that younger members of the profession will find it a valuable assistance in preparing designs, not only because they at once get the displacement very nearly correct, but also because, by basing the design on a fair bilge diagonal, they will be able to fair the ship more quickly than by commencing in any other way.

Particulars of Vessels Used in Constructing the Curve of Bilge Diagonal Co-efficients.

No. of Spot on Figure 1.	1	2	3	4	5	6	7	8	9
Length between perpendiculars	152′ 0″	220′ 0″	176′ 0″	390′ 0″	224′ 0″	270′ 0″	350′ 0″	264′ 0″	400′ 0″
*Length for displacement	144′ 0″	212′ 6″	168′ 0″	379′ 1″	216′ 0″	262′ 4″	340′ 3″	256′ 3″	390′ 0″
Breadth moulded	22′ 6″	32′ 0″	25′ 0″	42′ 0″	29′ 6″	35′ 9″	39′ 10″	33′ 0″	44′ 6″
Depth of hold	12′ 0″	17′ 6″	14′ 0″	32′ 9″	17′ 3″	23′ 0″	31′ 3″	22′ 3″	31′ 3″
Moulded draught	9′ 0″	12′ 0″	10′ 6″	20′ 0″	16′ 0″	20′ 0″	22′ 0″	18′ 0″	24′ 0″
Rise of floor at side	6′ 3″	1′ 0″	2′ 3″	2′ 0″	1′ 9″	0′ 6″	1′ 3″	1′ 0″	1′ 6″
Displacement in tons	334	1460	752	6047	1919	4082	6436	3260	8817
Displacement co-efficient	·4007	·6262	·5950	·6646	·6620	·7617	·7556	·7500	·7410
Area of load-line	1165	5335	3282	12989	5265	8325	11860	7340	15226
Co-efficient of load-line	·7190	·7846	·7810	·8220	·8240	·8876	·8803	·8680	·8913
Area of midship section	124	358	228·5	771	432	699·8	843	564	1000·5
Co-efficient of midship section	·6120	·9334	·8700	·9190	·9140	·9801	·9622	·9500	·9474
Co-efficient of mid. sec. cylinder	**·6549**	**·6709**	**·6860**	**·7240**	**·7200**	**·7763**	**·7852**	**·7900**	**·7908**
Area of bilge diagonal	1067	5194	3288	14113	5952	10312	14586	8959	17950
Co-efficient of bilge diagonal	**·6920**	**·7048**	**·7210**	**·7446**	**·7490**	**·7941**	**·7996**	**·8020**	**·8046**
Centre of buoyancy (Abaft midship section)	3·875′	1·510′	·804′	3·994′	1·000′	·872′	1·885′	·277′	1·330′
Centre of load-line (Abaft midship section)	4·890′	3·890′	2·820′	5·615′	3·940′	4·918′	7·508′	4·120′	6·630′
Centre of bilge diagonal (Abaft midship section)	4·150′	1·080′	1·380′	1·891′	·828′	·258′	1·671′	·043′	·682′

No. of Spot on Figure 1.	10	11	12	13	14	15	16	17
Length between perpendiculars	160′ 0″	240′ 0″	270′ 0″	285′ 0″	245′ 0″	235′ 0″	286′ 0″	240′ 0″
*Length for displacement	154′ 3″	233′ 2″	264′ 0″	278′ 9″	238′ 0″	228′ 0″	278′ 0″	233′ 0″
Breadth moulded	23′ 0″	34′ 0″	35′ 0″	35′ 0″	33′ 0″	31′ 0″	34′ 6″	33′ 0″
Depth of hold	11′ 0″	15′ 6″	24′ 6″	24′ 6″	21′ 0″	19′ 0″	24′ 3″	20′ 2″
Moulded draught	10′ 0″	15′ 0″	20′ 0″	20′ 0″	18′ 0″	16′ 0″	20′ 0″	17′ 6″
Rise of floor at side	0′ 4″	0′ 6″	1′ 2″	2′ 0″	0′ 6″	0′ 9″	1′ 9″	1′ 0″
Displacement in tons	773	2581	3929	4202	3153	2494	4142	3025
Displacement co-efficient	·7626	·7598	·7440	·7537	·7806	·7718	·7558	·7870
Area of load-line	3086	6973	7681	8498	6805	6130	8385	6712
Co-efficient of load-line	·8837	·8821	·8500	·8710	·8640	·867	·8740	·8500
Area of midship section	222	489·66	659·6	655	572·7	473	647	550
Co-efficient of midship section	·9652	·9600	·9420	·9357	·9640	·9540	·9380	·9530
Co-efficient of mid. sec. cylinder	**·7901**	**·7913**	**·7897**	**·8050**	**·8090**	**·8094**	**·8050**	**·8260**
Area of bilge diagonal	3435	7648	9925	10608	8705	7493	10644	8562
Co-efficient of bilge diagonal	**·8069**	**·8069**	**·8085**	**·8165**	**·8160**	**·8160**	**·8200**	**·8314**
Centre of buoyancy (Abaft midship section)	·916′	—	·880′	1·290′	1·025′	1·150′	1·112′	1·088′
Centre of load-line (Abaft midship section)	2·150′	3·160′	3·960′	4·380′	2·734′	2·789′	4·300′	2·407′
Centre of bilge diagonal (Abaft midship section)	·150′	·360′	·550′	·617′	·633′	·674′	·440′	·660′

* The "Length for Displacement" is used in calculating the co-efficients.

APPENDIX.

SINCE writing this paper I have had an opportunity of applying my method to sailing vessels, and I may state that the co-efficients are got in the same manner as with steamers, and the same bilge diagonal diagram can be used, the only difference being in the alteration of the parabola. This line is not to be altered as shown on the plate, but a slice is to be cut off the after-end and another of the same area put on the fore-end, the slice being larger or smaller according to the proportions of the vessel and the taste of the designer.

	Sailing Vessel.
Length between perpendiculars ...	282 ft.
Length for displacement ...	276 ft.
Breadth moulded ...	42 ft. 6 in.
Depth of hold ...	24 ft.
Moulded draught ...	21 ft.
Rise of floor at side ...	2 ft. 6 in.
Displacement in tons ...	4,620
Displacement co-efficient ...	·6560
Area of load line ...	9,540
Co-efficient of load line ...	·8140
Area of midship section ...	820
Co-efficient of midship section ...	·9410
Co-efficient of mid-section cylinder ...	·7140
Area of bilge diagonal ...	10,150
Co-efficient of bilge diagonal ...	·7380
Centre of buoyancy abaft midship section ...	—3·870 ft.
Centre of load line abaft midship section ...	·300 ft.
Centre of bilge diagonal abaft midship section ...	—2·850 ft.

DISCUSSION.

Mr. W. H. WHITE: I simply desire to say that I think all those who have had any experience in designing will thank Mr. Zimmermann for giving us so freely the result of his experience, and putting it in a form which admits of trial and use by anyone who desires to do so. I think papers of this kind, although short, have a material value when placed in our Transactions.

Mr. W. JOHN: I add to that this fact, that I know Mr. Zimmermann practises it himself in our drawing office, where he is our chief draughtsman, and the marvellously short space of time in which he can get out a set of lines to the required displacement is a sufficient testimony, even if I had not my own experience as a draughtsman to fall back upon to judge of the merits of this paper, to convince me that there is a marvellous facility in the process, which I am sure any young draughtsman will do well to study, and try and practise, and if he does so I am sure he will get benefit out of it.

Mr. J. HAMILTON, Jun.: I would only say one word with regard to this paper. The author says that he understands that shipbuilding firms of repute, even on the Clyde, are not in the habit of making models. I think, whatever may have been the case in the past, that there can scarcely be a shipbuilder on the Clyde, or almost anywhere else, who does not prepare a set of lines, although he may depart somewhat from it. Mr. Zimmermann may know better than I do, but I fancy there is nobody on the Clyde who does not design a ship on paper before he makes the model.

Sir EDWARD REED: I should like to say a word. When I heard the first paragraph of this paper, I wondered what sort of a paper it was, or where Mr. Zimmermann came from, because to say in these days men build ships without making models at all, or making drawings, and without making any calculation in connection with them, does sound a little strange; but the justification for the paper is to be found in the last paragraph. When you consider that the load water-line of a ship, or any other water-line, furnishes a very poor indication of what a ship's displacement would be, and that the vertical section forms a still worse, and that the bilge diagonal section forms a very good average indication of the displacement, I have not the smallest doubt that the system has value in it, and the testimony Mr. John bears justifies that view completely. I think that we may fairly and fully join in the expression of thanks which has been accorded to Mr. Zimmermann for having produced this paper.

Mr. ZIMMERMANN: Seeing that the evening is so far advanced, I think there is nothing for me to say, except to express my thanks for the kind reception you have accorded to my first attempt.

The PRESIDENT: I am quite sure you will allow me to convey your united thanks to Mr. Zimmermann for his paper. I may remind you that he was only elected on Wednesday last, and coming forward with a useful paper of this description shows that he is perfectly prepared to devote his evident abilities and energies to the advantage of this Institution. Gentlemen, I think that concludes our business.

CONCLUDING PROCEEDINGS.

Sir EDWARD REED: Will you allow me the privilege of proposing a resolution? Gentlemen, I am sure we should be very sorry to separate, although the hour is rather late, without expressing our thanks to the President, not only for his presence and close attendance at these meetings, but for the admirable manner in which he has presided over our proceedings. I confess that, not having the noble Earl in my mind, when we lost the services of our late President, I had very great misgivings whether we should ever again succeed in getting a President who was at once so punctual, so loyal to the rules of the Institution, and so fraught—if I may use the phrase—with intelligent criticism, as well as proper control over our proceedings, as was Lord Hampton. We felt that during a long series of years, and I think the votes of thanks which were presented to Lord Hampton at the end of our meetings were amongst the sincerest part of our work. I am sure you will bear me out when I say that in none of those essential qualities of a President is our present noble President wanting. In obtaining the patronage and friendship and the presence of Lord Ravensworth, we have been made to feel no loss in the loss even of our late President. Nobody can be more punctual; sometimes I wonder whether his Lordship is not a little too punctual, because it is sometimes difficult for the Council to avoid being a few minutes behind time. He has been very constant in his attendance, although, of course, he is necessarily here at a very great sacrifice during these long sittings, which are repeated twice a day. I am sure we have all profited, as well as been deeply interested by the remarks which have fallen from his Lordship, both in opening the meeting and during the discussions that have occurred; therefore with the utmost confidence I ask you to join me in the most cordial and grateful vote of thanks to his Lordship for his services to this Institution this year.

Mr. B. MARTELL: I have very great pleasure in seconding this resolution: I am sure you will cordially join in it. The more we have seen of Lord Ravensworth, our noble President, the more we are enabled to judge of his qualities and abilities for filling the office he does in succession to the able President who presided over us before. His courteousness of manner, his great tact in dealing with all questions coming before him here, and his especial knowledge on subjects pertaining to naval architecture, and, in fact, his great and varied knowledge of all subjects coming before him, especially qualify him for fulfilling the duties of the office in the able manner he does; and I am sure you will all agree with me that it is a source of great gratification to us all, and a great benefit to this Institution, that we have been enabled to secure his services as President of this Society. Gentlemen, I am sure you will join heartily in a vote of thanks to his Lordship.

The resolution having been put to the meeting was carried unanimously.

The PRESIDENT: Gentlemen, you can readily understand that it is difficult for me sufficiently to express my gratitude for this marked expression of your favour, and for the extremely kind terms —far too kind terms—in which my friend Sir Edward Reed, and my friend Mr. Martell, have been

pleased to propose and second this proposition. Gentlemen, my task really is rendered so easy by your cordial assistance, and acquiescence in any proposition that I have felt it my duty to make in order to maintain proper order and obedience to our rules, that really as far as discipline goes—because the Chair is charged with the maintenance of discipline—it is almost a sinecure. I have nothing to do but to ask for your opinion. You give it with perfect willingness, and in a manner which conveys entirely to me your wishes. As long as you do that, I shall have no difficulty whatever in presiding over this Institution, because, of course, in large assemblages of this kind, where great intellects are brought into contact, gentle passages at arms will occur from time to time. It is the object of these institutions to come into conflict, in order to get at the truth; but I always find that knights who are gallant, however fierce they may appear for the moment, are perfectly willing to shake hands the moment the thing is over, and walk away arm in arm. Nothing can be more satisfactory than that. There is another feature I should mention, because I think it is desirable I should do so. I think that perhaps this present meeting will stand as high as any previous meeting in the annals of this Institution. We have had papers read upon subjects which are exercising the public mind in the highest possible degree, and those papers have been read and discussed with all the talent and the research that could be brought to bear upon them; but what is to my mind more satisfactory than all is that a great—I think the greatest—number of these papers have been in the hands of young men. My encomiums are worth nothing, but still I think they are due to our young members. They show an immense amount of study and research; some of them are so tremendously difficult that I cannot follow them; but still it is for the benefit of science and scientific men to understand them. But the great thing is this, that the circulation of these papers will show the nation to a very large extent the reserve of talent that there is in this country. It is valuable for the great maritime interests in this country that they should possess that knowledge, and should know where to lay their hands upon eminent men, it will be to their advantage that they should do so, and I trust it will be for the advantage of the young men also in after life that they should rise to those positions of eminence which so many of our young members have already attained. I thought it right to make this one allusion, not so much by way of encouragement, but as a tribute of gratitude to our young men for the part they have taken in these proceedings. One word more, and one word only. Gentlemen, I should be nowhere if it were not for the aid of our excellent Secretary. It is impossible to overrate the value of the services of a gentleman such as Mr. Holmes. His ability is great, his knowledge is great, and his tact is quite unequalled; and therefore I feel that any praise you have been pleased to confer upon me ought to be largely shared by your excellent and able Secretary, Mr. Holmes. I thank you with all my heart for the kind manner in which you have been pleased to acknowledge my efforts.

Mr. Wigham Richardson: My Lord, I have great pleasure in proposing a vote of thanks to the gentlemen who have been so kind as to give their services, and to act on the Council of this Institution. We were all very well pleased to see from the Report that the good ship is getting in every way into good trim, and is likely to make a long and prosperous voyage owing to the unfailing patience and courtesy of the captain, and united to that, as far as I have experienced, that of all the other officers. I hope you will excuse me saying much more on the subject, but will cordially pass this vote of thanks. Allow me to make one more observation, and that is as regards the finances. I think that something might be done (and I think our noble President alluded to it at the dinner) to increase the number of members, and especially of Associates. At one of the evening meetings we had a distinguished barrister here. (I don't know whether he was known to many of you, and I

do not know that he found the proceedings particularly interesting, they happened to be particularly dull that evening; but he is the author of a recent work on Admiralty Law, and is one of the highest authorities on shipping law in the country.) I went out with him, and he remarked that many years ago I suggested his becoming an Associate, and that the Transactions had been a volume that he looked forward to every year, and, as he kindly put it, they had been worth many times over the small subscription which he had had to pay. In other instances I have found that gentlemen who are interested in shipping matters would be more than pleased to be elected as Associates, and their coming here would add freshness to our discussions, but they have not known how to obtain that position. Once more I propose that a vote of thanks be awarded to the Council.

Mr. J. Edwards: I have much pleasure in seconding that, my Lord.

[The resolution having been put to the meeting was carried unanimously.]

Mr. E. Crofton: I have now the pleasing duty of proposing a vote of thanks to the Society of Arts for so kindly allowing us to meet year after year in this most convenient hall. Further, it appears to me that our thanks are all the more due to our hosts if we bear in mind the fact that of necessity the hospitality which they extend to us must sometimes entail more or less of inconvenience upon themselves.

Mr. W. John: My Lord, I beg to second that proposition. I think it is a right thing that every year we should be reminded of the fact that we do owe a debt of gratitude, and a very deep one, to the Society of Arts for their kindness. I think that we sometimes otherwise would begin to think that this hall belonged to us as much as it did to the Society of Arts. We are really, gentlemen, so much at home that the Institution would feel like a fish out of water elsewhere. I cordially second that proposition.

The President: Gentlemen, I am a member myself of that body, and I am as conscious as anybody here of the almost inestimable favour they confer upon us by granting to us this most convenient and handsome hall, and I hope you will unanimously join in the vote of thanks.

[The resolution having been put to the meeting was carried unanimously.]

The President: Gentlemen, that concludes our business, and I wish you all a very good night, and hope we shall all meet again another year.

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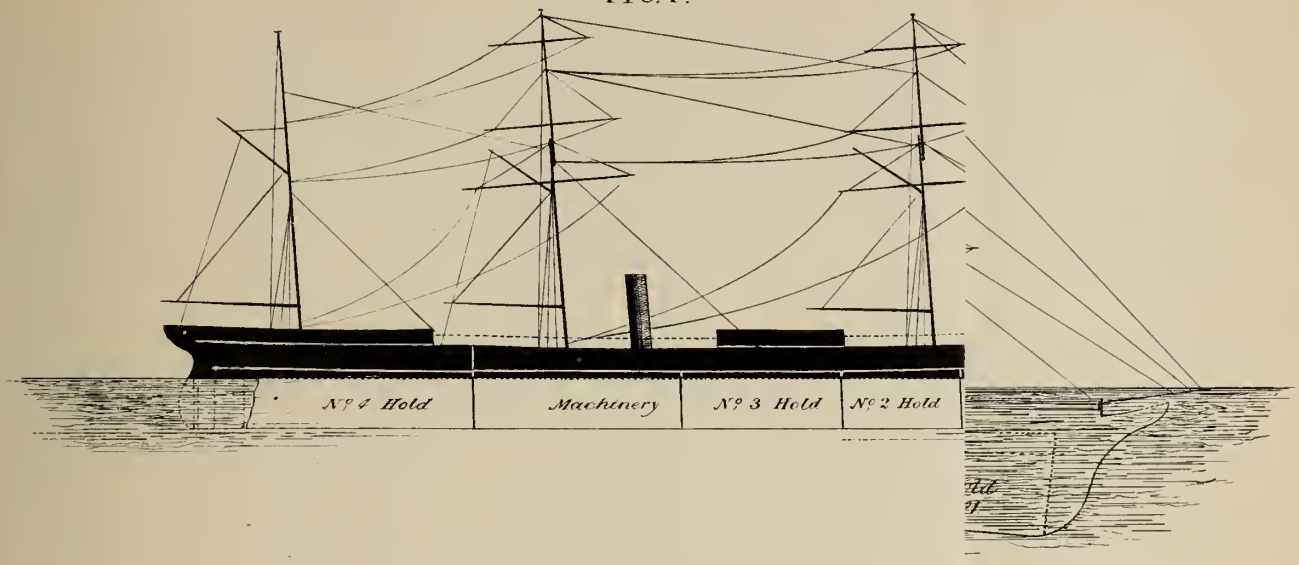

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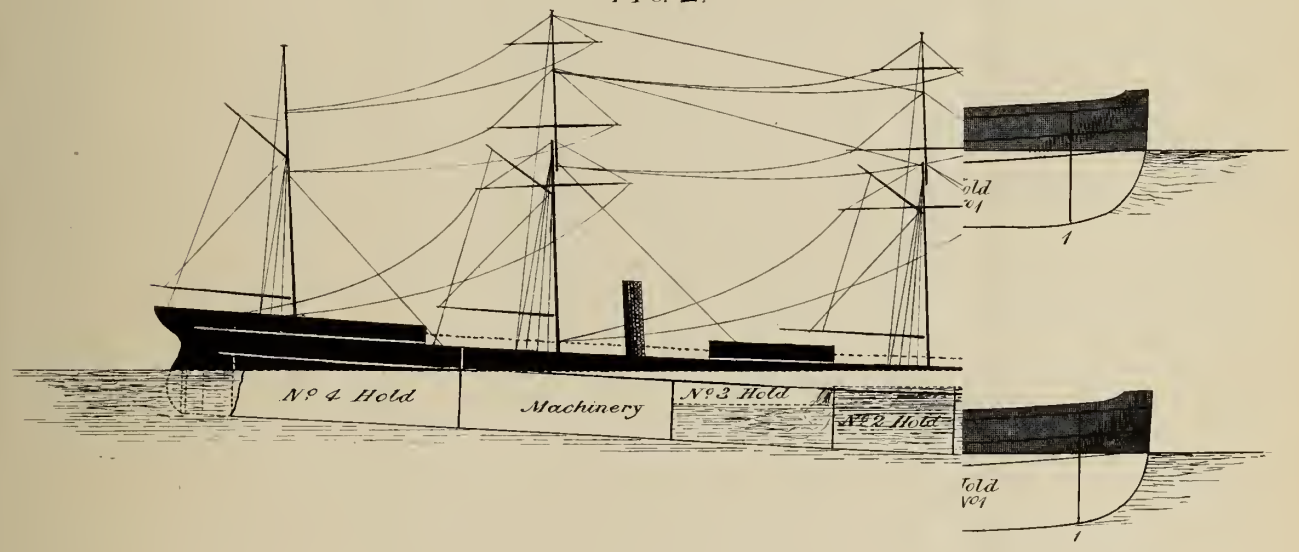

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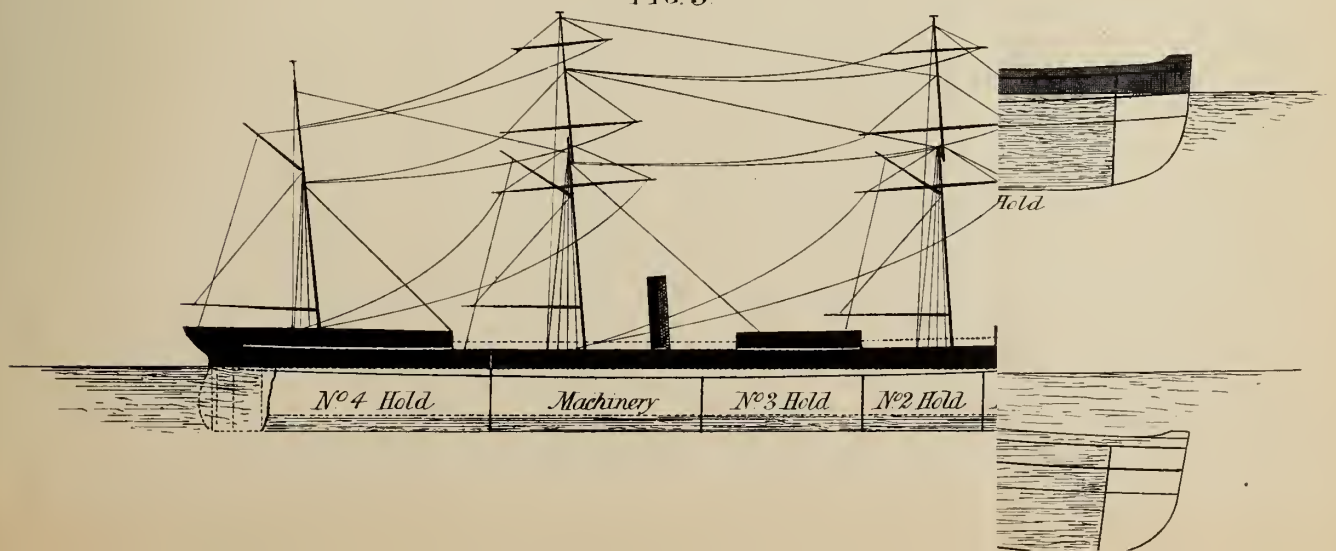

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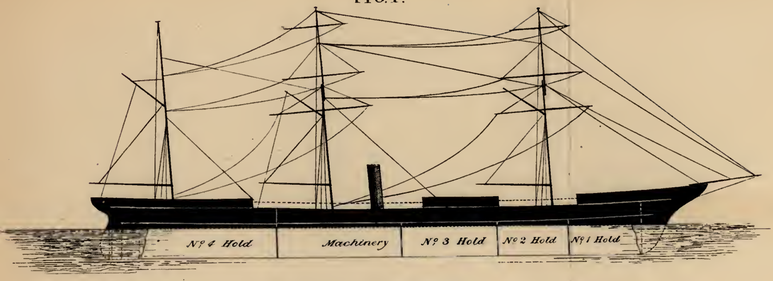

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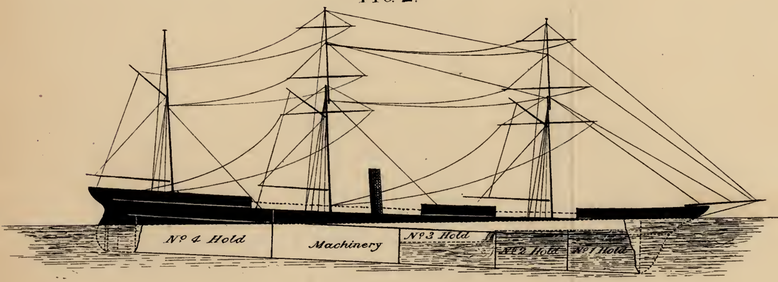

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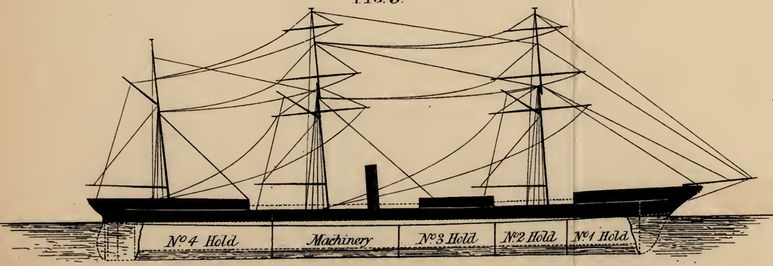

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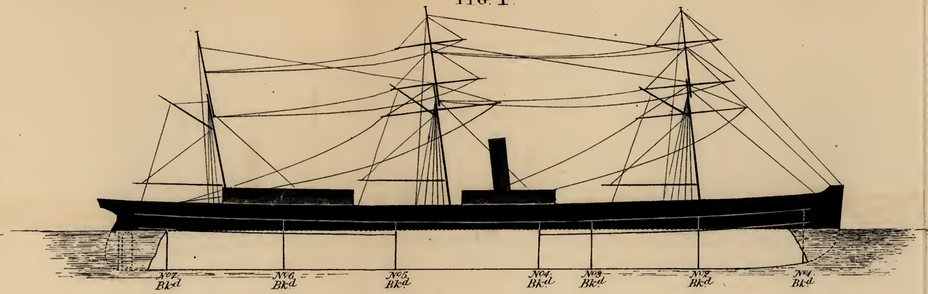

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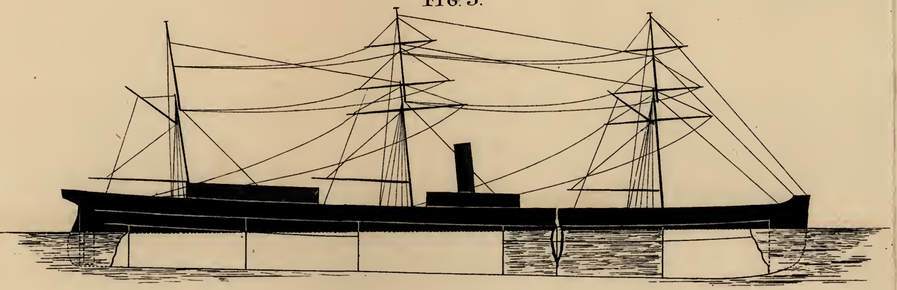

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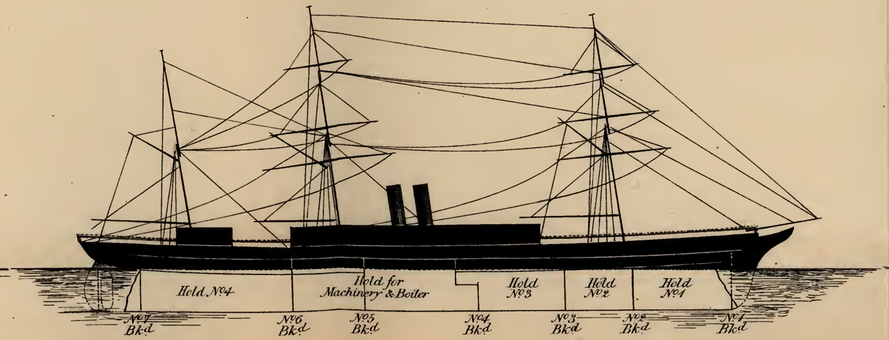

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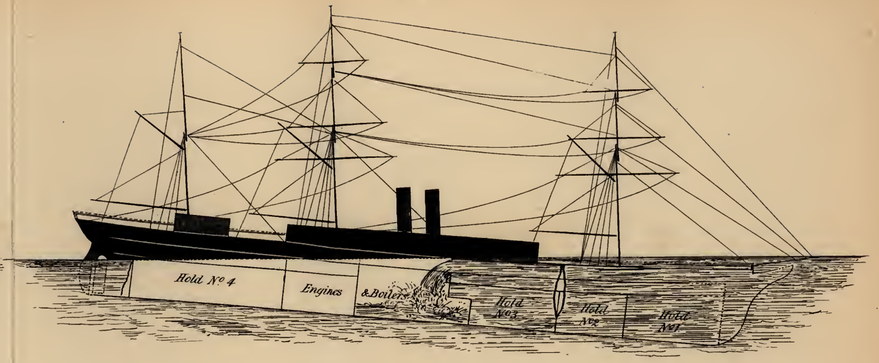

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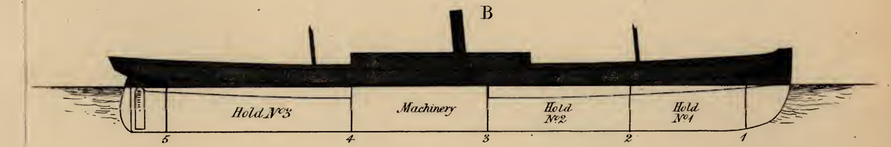

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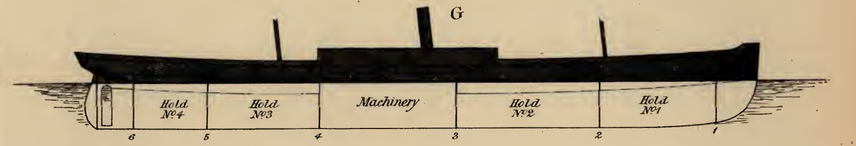

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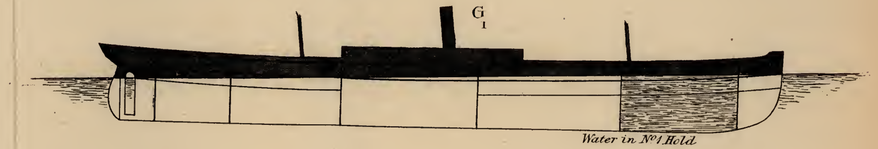

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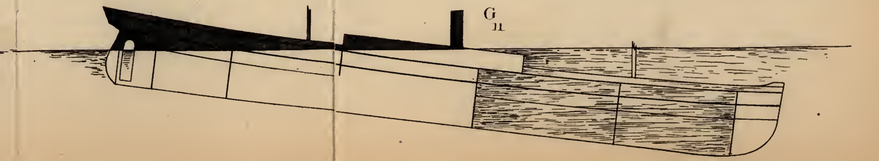

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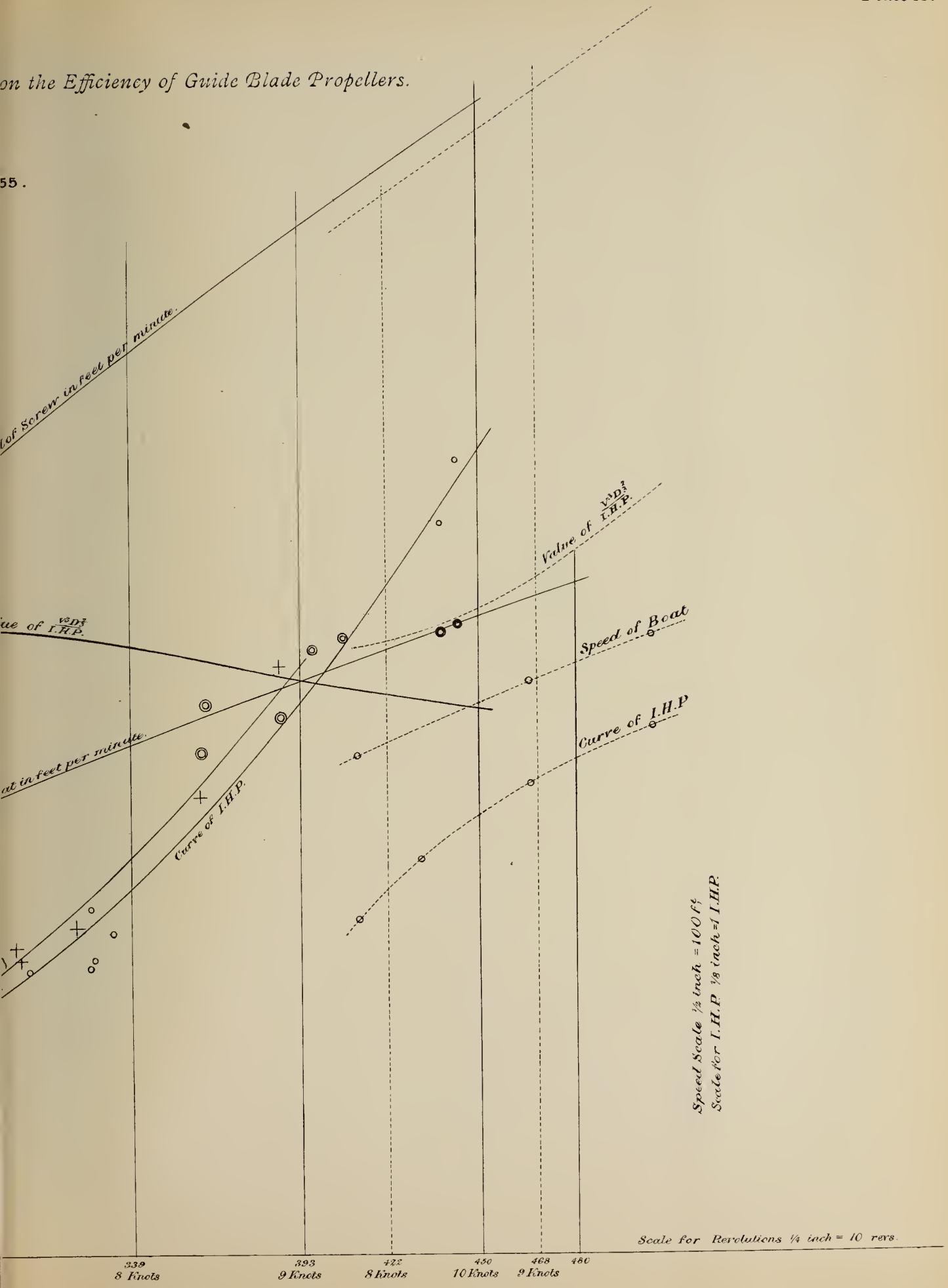

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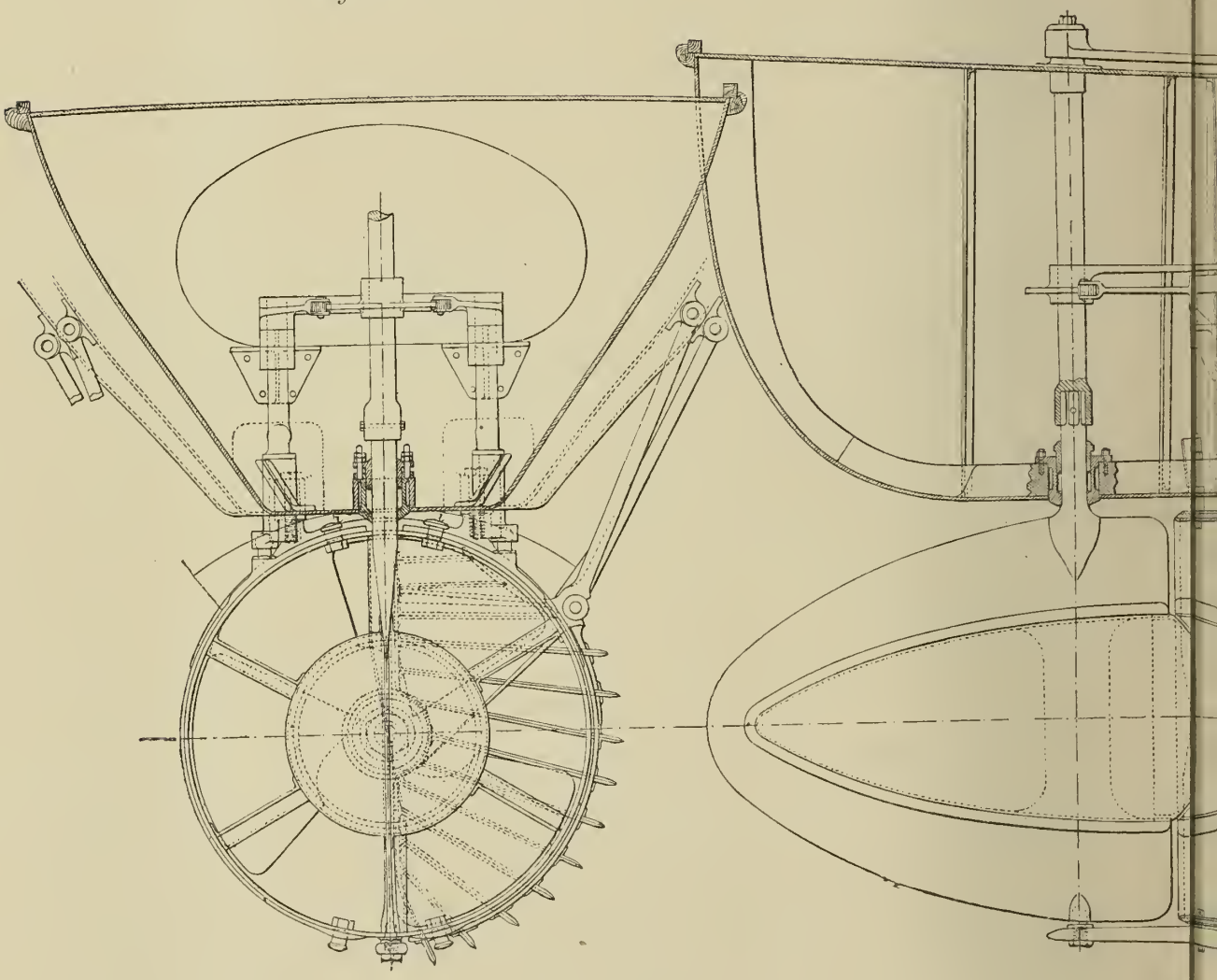

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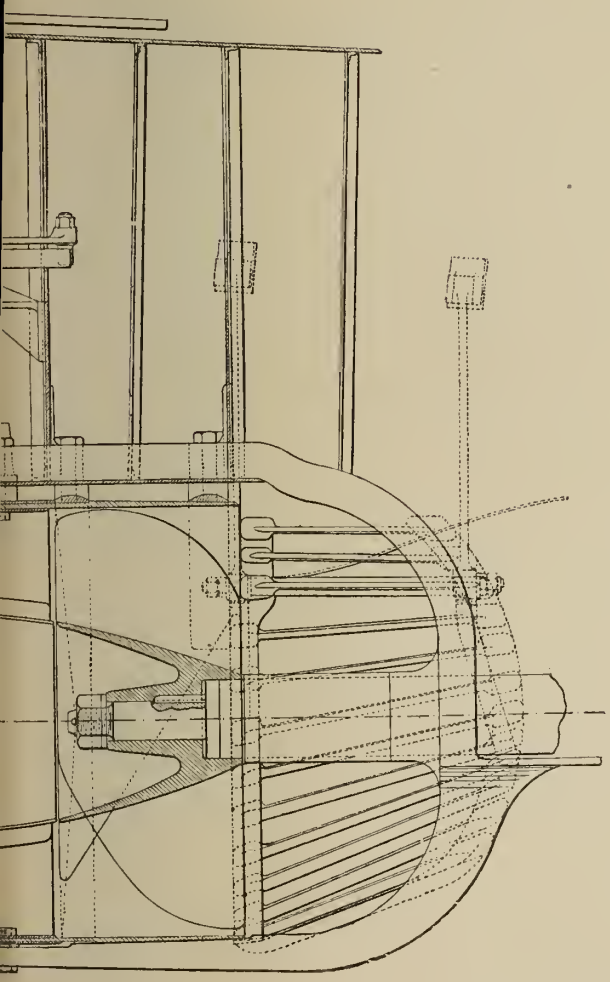

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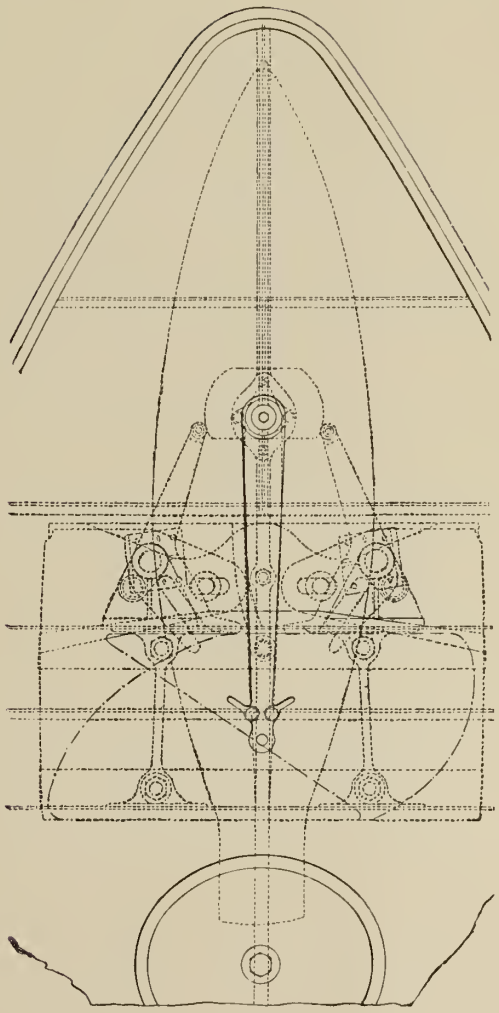

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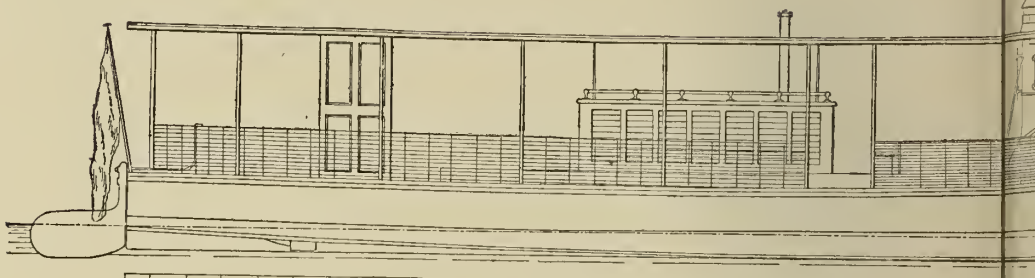

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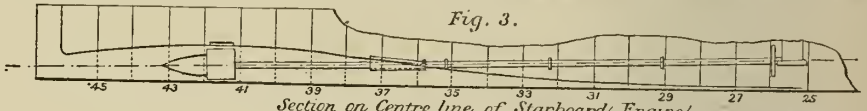
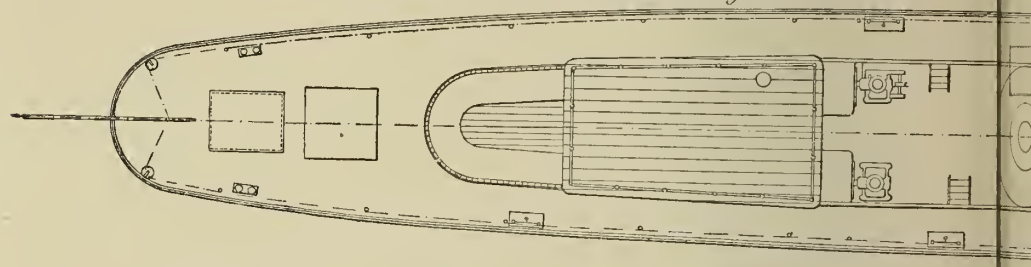


Section on Centre line of Starboard Engine



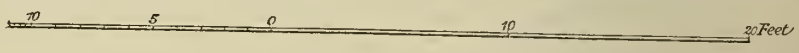
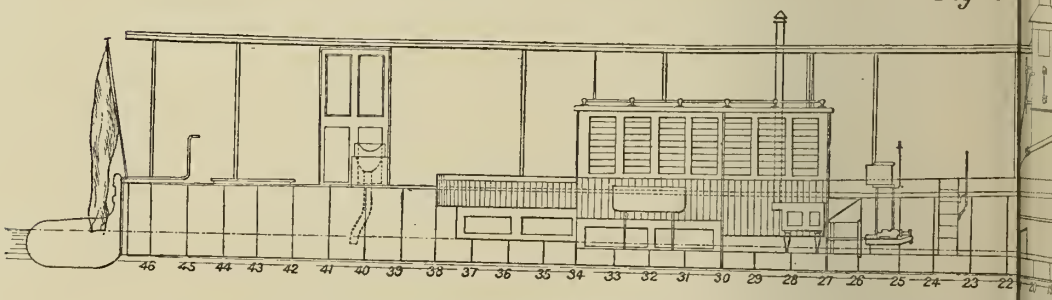
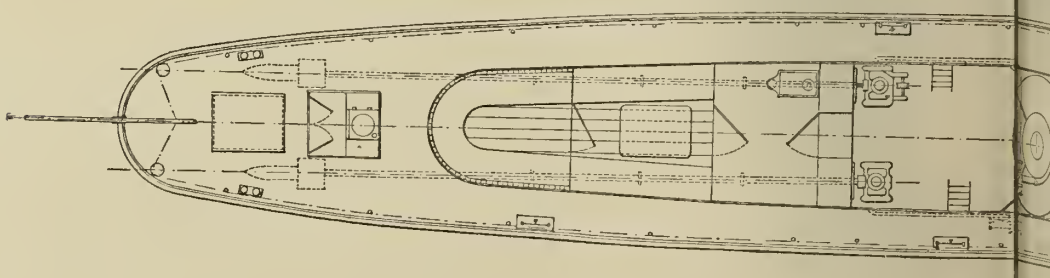
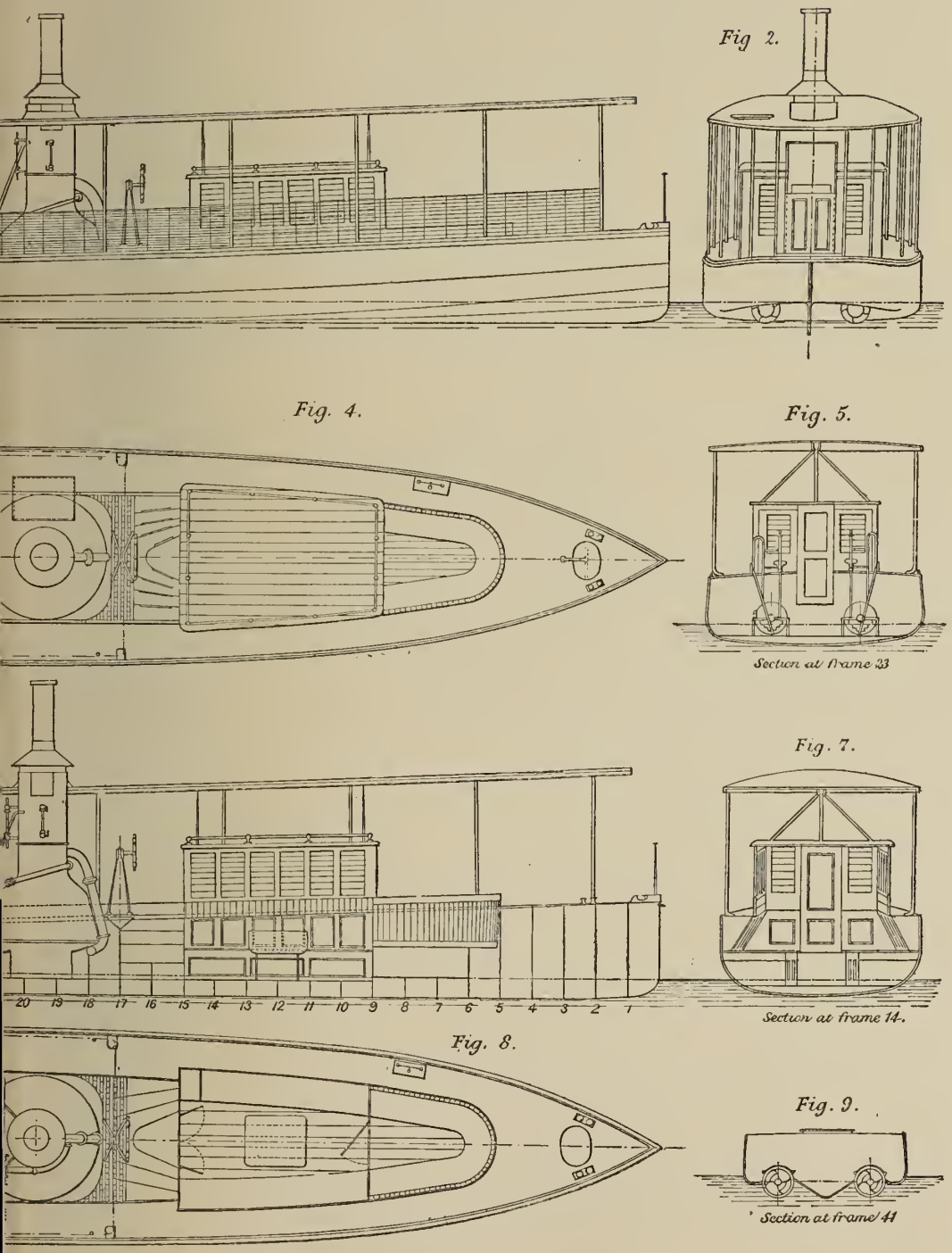


Fig. 6





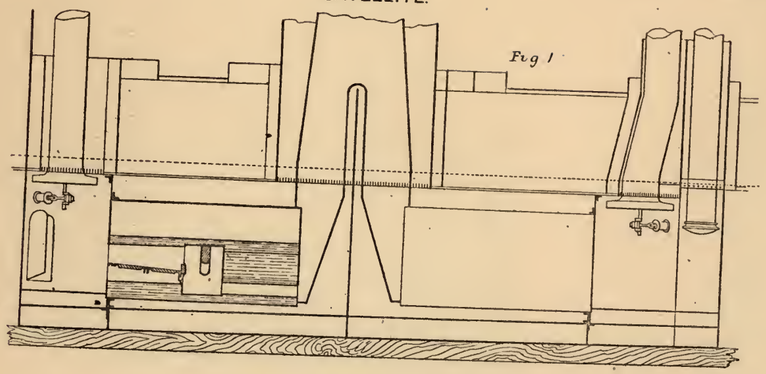
on the Efficiency of Guide Blade Propellers.



To Illustrate Mr. R. J. Butler's Paper on the Steam Trials of the Satellite and Conqueror under Forced Draught.

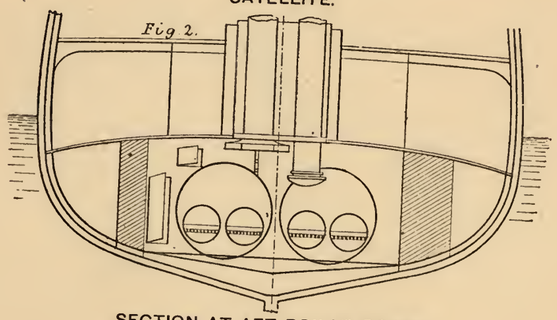
SATELLITE.

Fig 1



SATELLITE.

Fig 2.



SECTION AT AFT BOILER ROOM.

Fig 3.

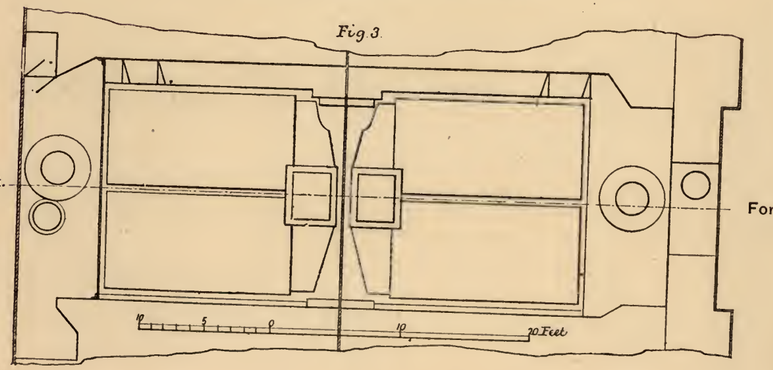


Fig. 4.

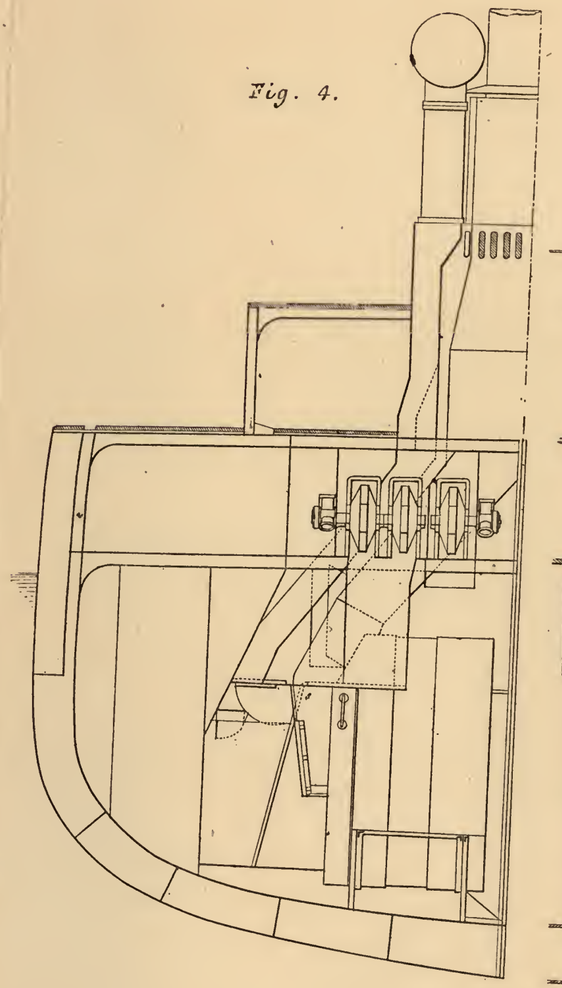
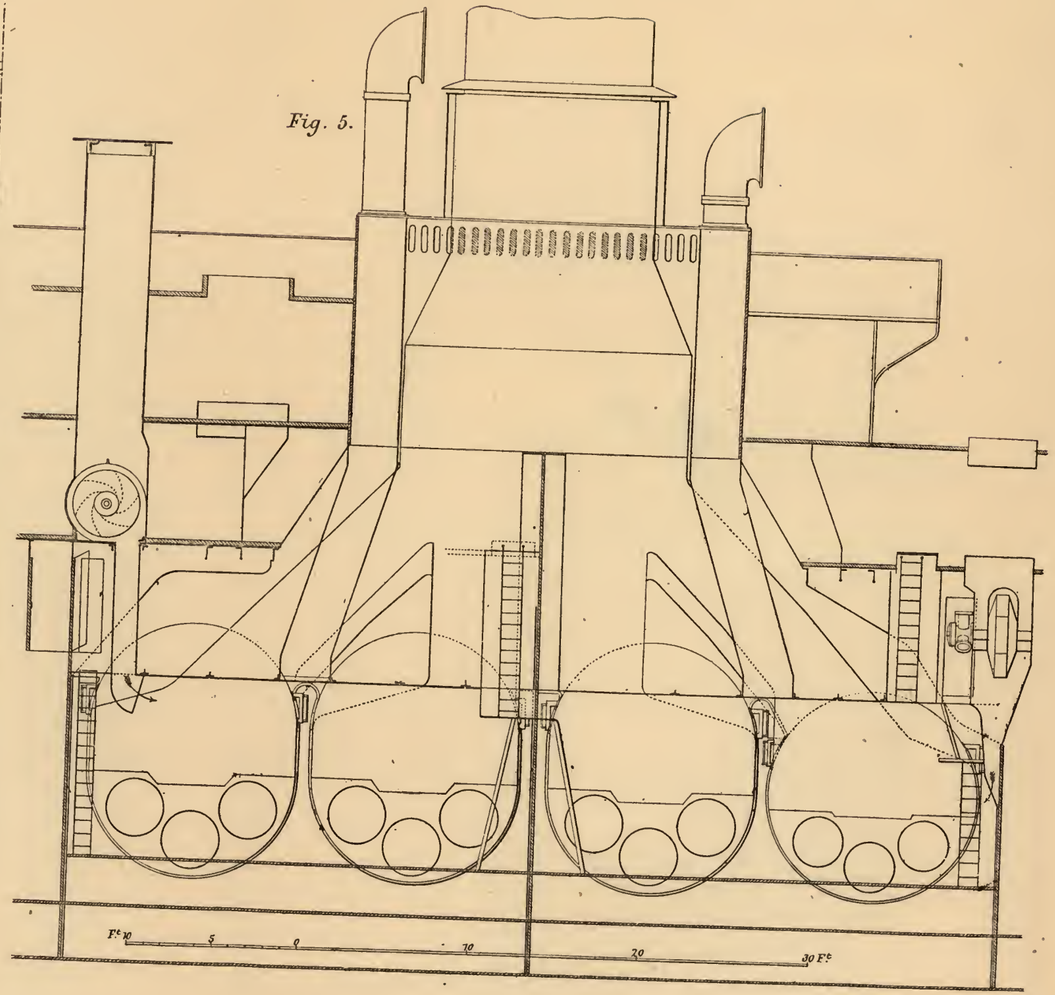
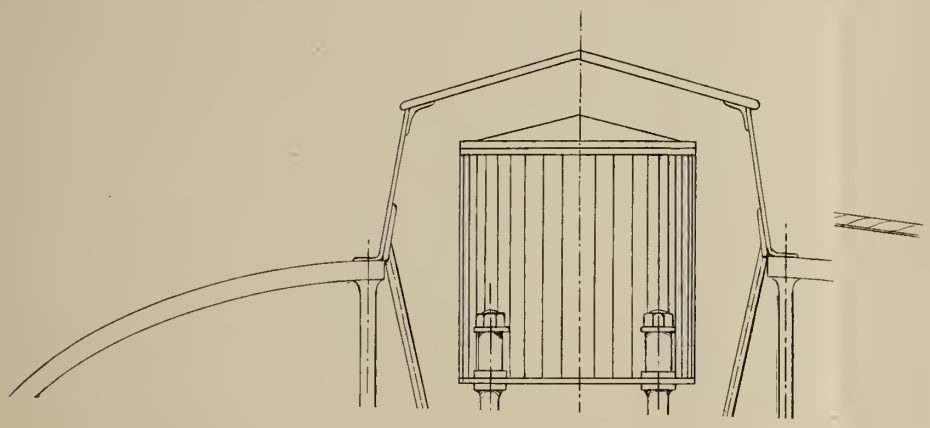
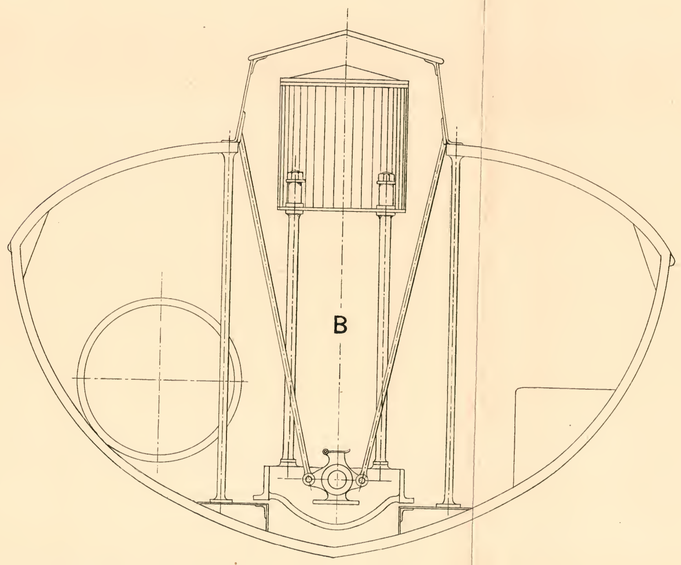


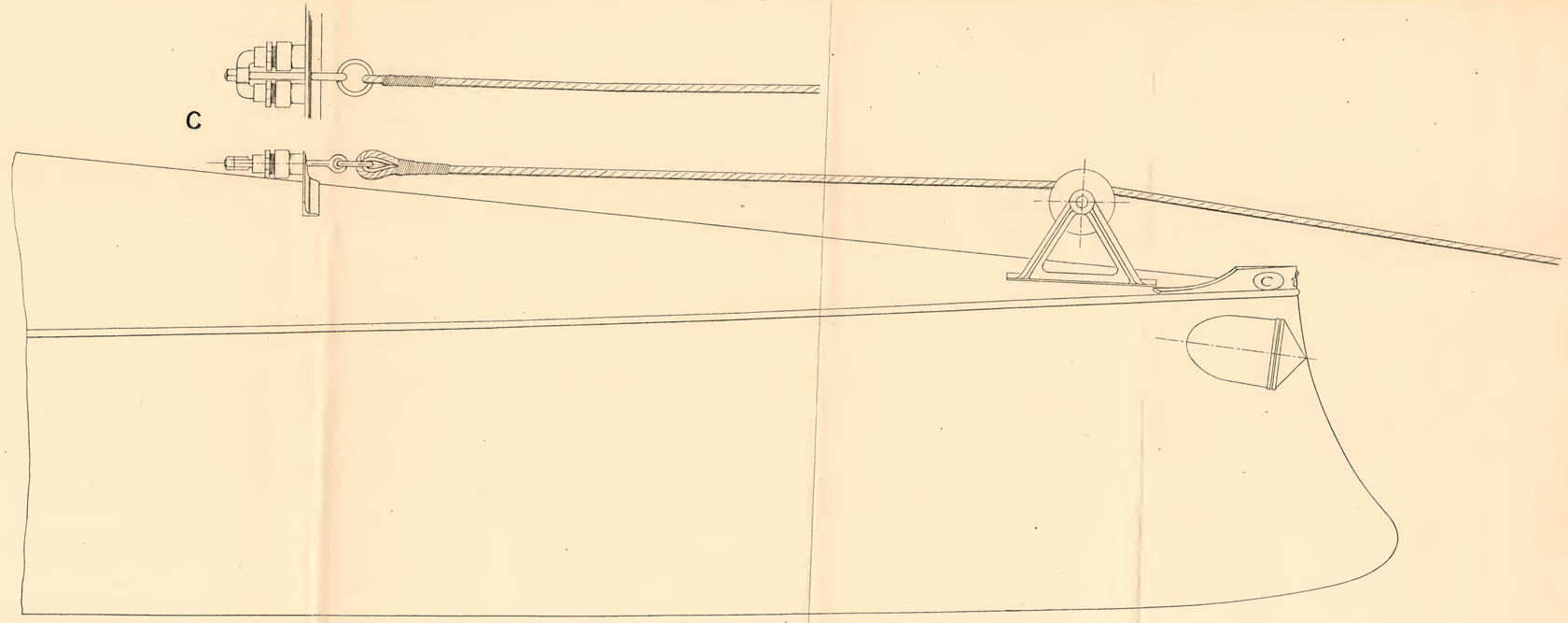
Fig. 5.

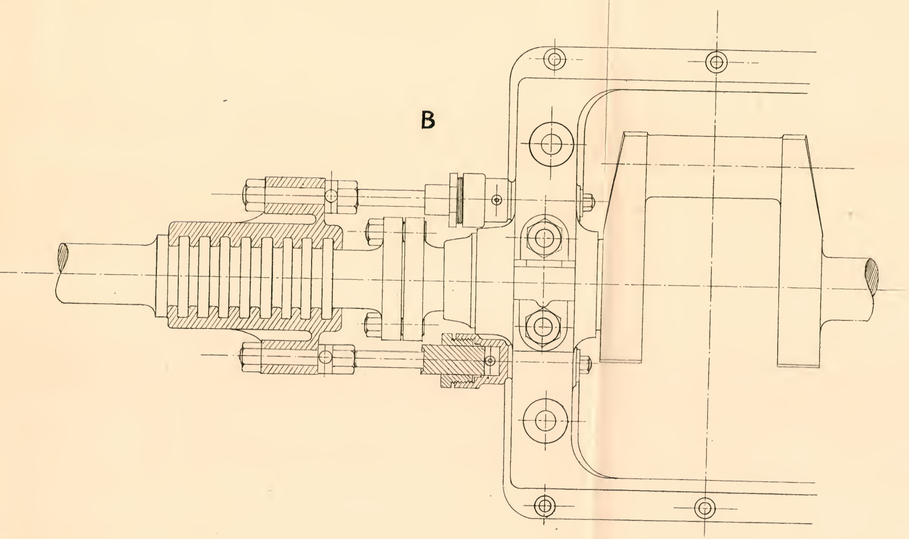




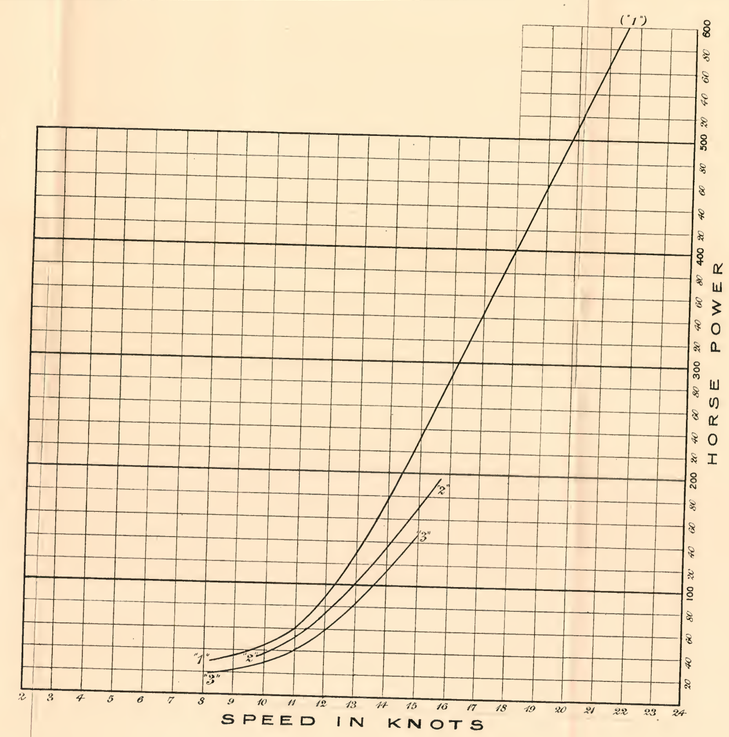
To Illustrate Mr. A. F. Yarrow's Paper on Some Experiments to Test the Resistance of a first-class Torpedo Boat.





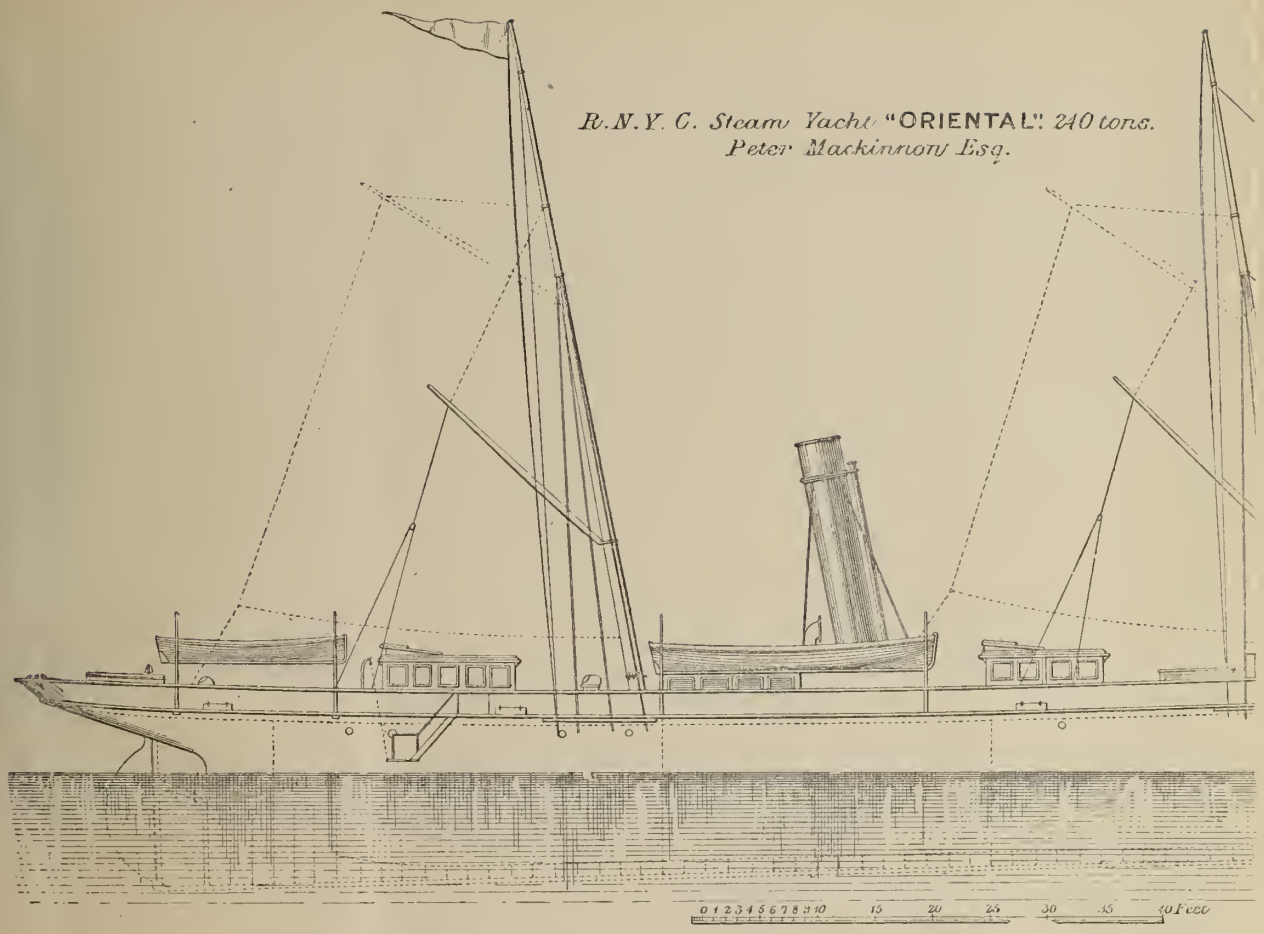


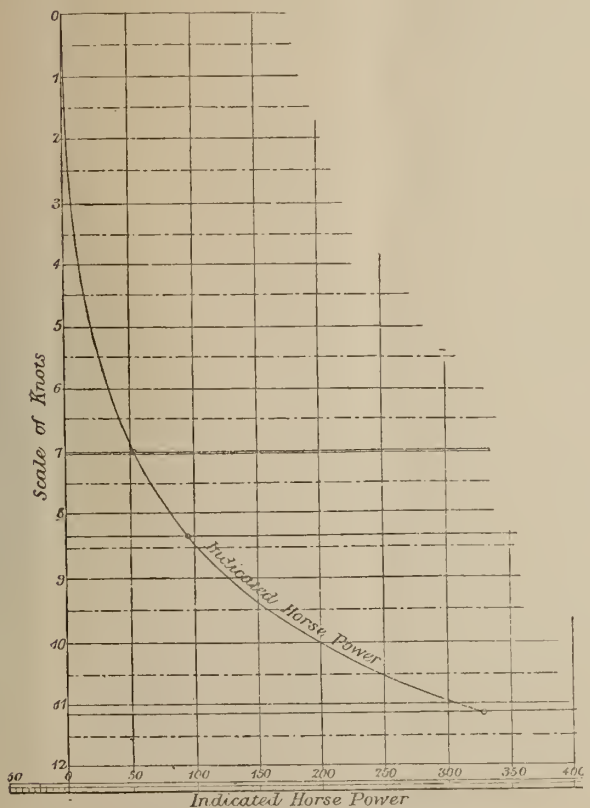
A



A

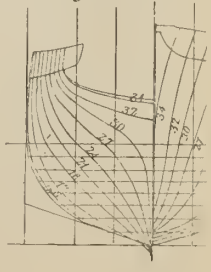
R.N.Y.C. Steam Yacht "ORIENTAL". 240 tons.
Peter Mackinnon Esq.





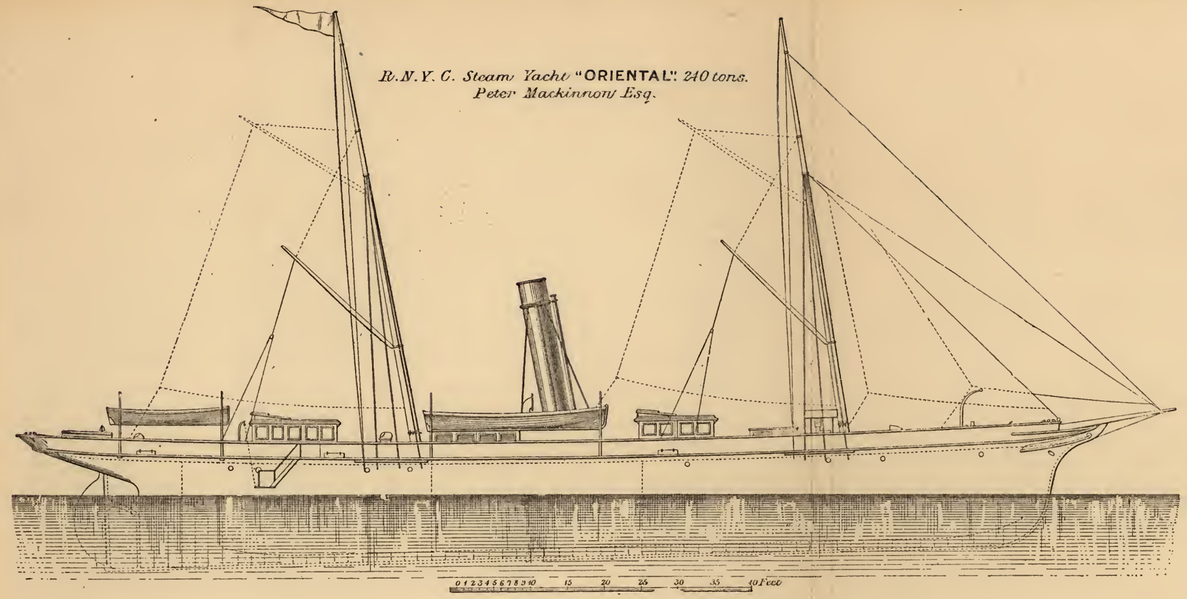
Speed Diagram "ORIENTAL".

Body Plan "OR

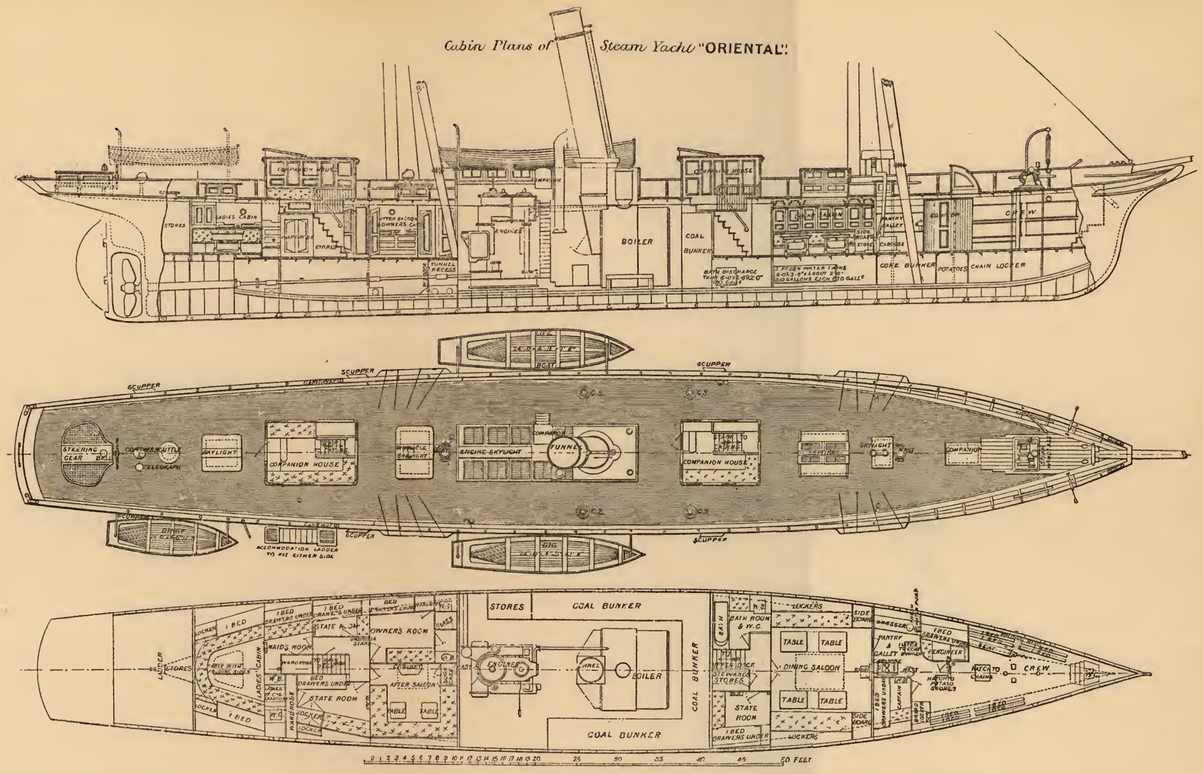


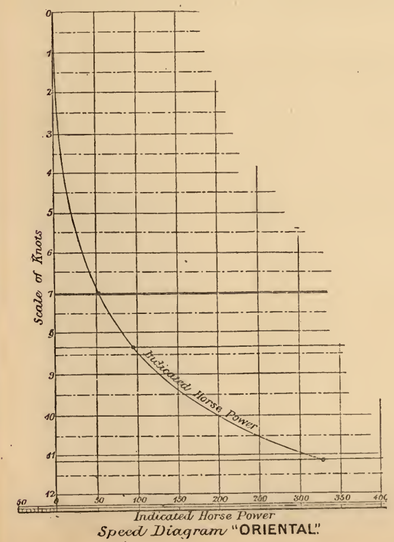
To Illustrate Mr. Dixon Kemp's Paper on Steam Yachts.

R.N.Y.C. Steam Yacht "ORIENTAL". 240 tons.
Peter Mackinnon Esq.



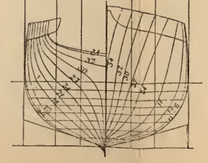
Cabin Plans of Steam Yacht "ORIENTAL".



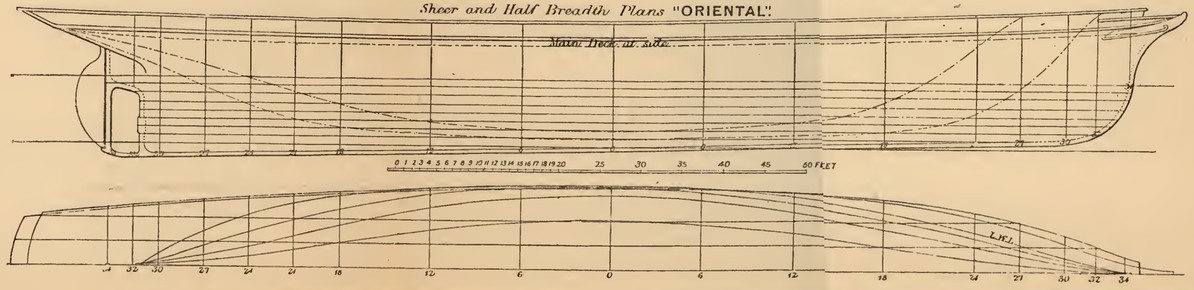


Speed Diagram "ORIENTAL".

Body Plan "ORIENTAL".



Sheer and Half Breadth Plans "ORIENTAL".



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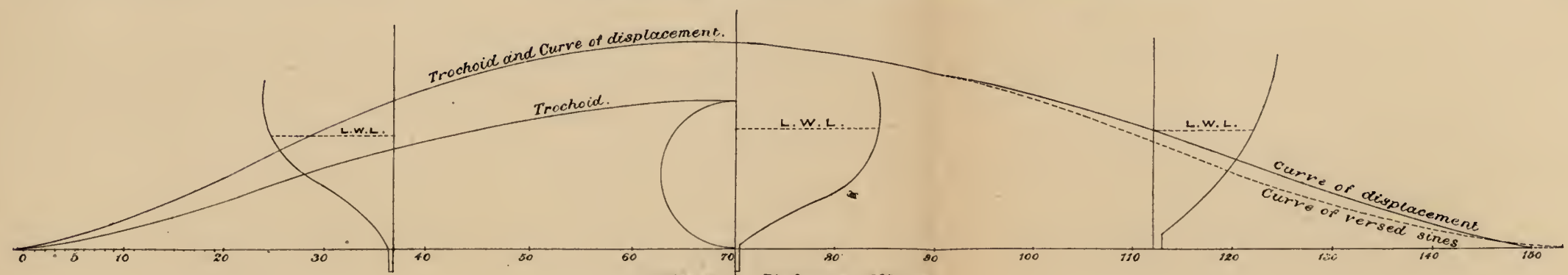
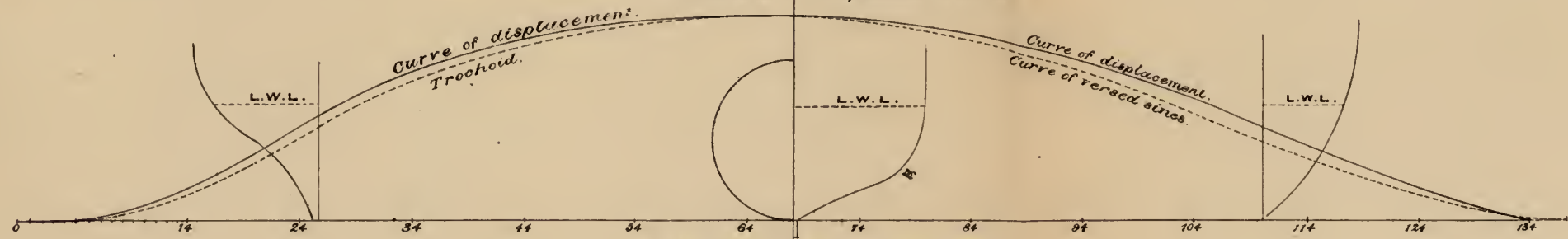
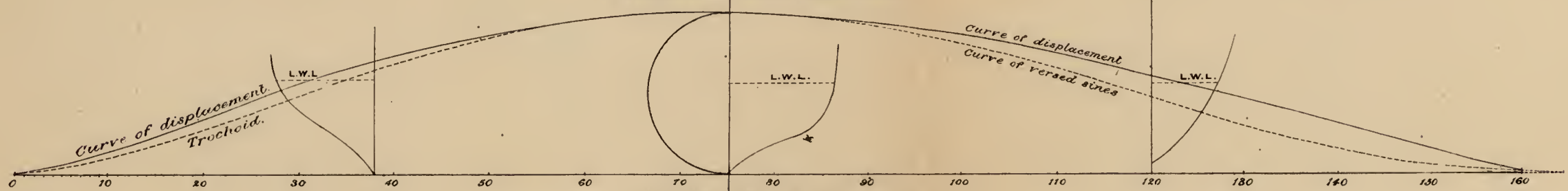
To Illustrate Mr. Dixon Kemp's Paper on Steam Yachts.



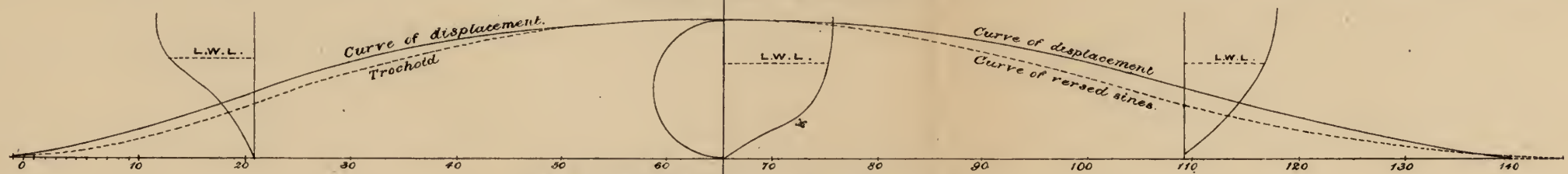
CHAZALIE Displacement 533 tons.



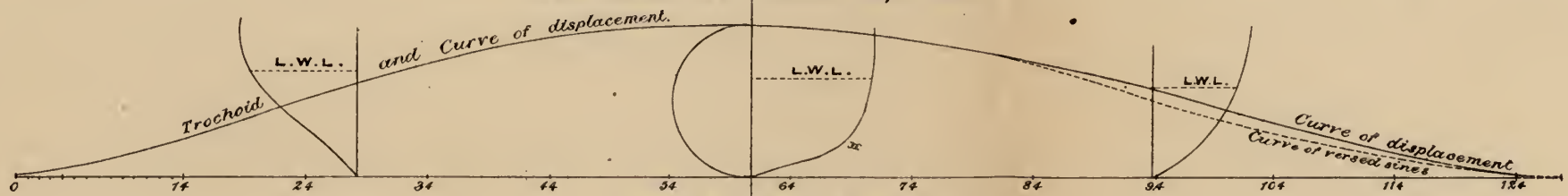
MARCHESA 455 tons displacement.



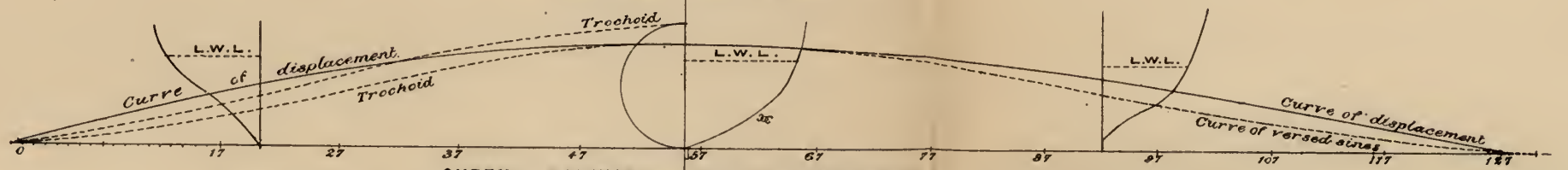
JACAMAR 422 tons displacement.



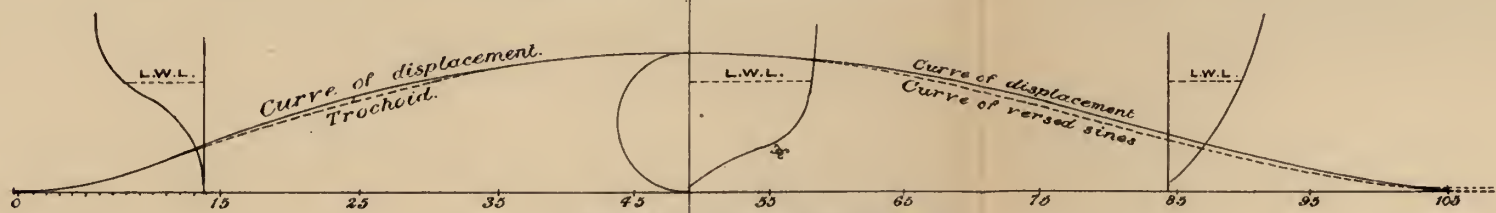
FAIR GERALDINE 315 tons displacement.



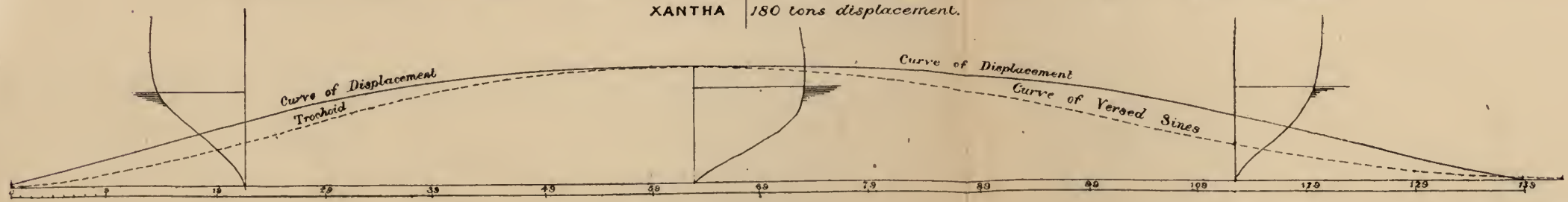
ORIENTAL 266 tons displacement.



QUEEN OF PALMYRA 210 tons displacement.



XANTHA 180 tons displacement.



"ARIES" 283 tons displacement.

Tr

To Illustrate Mr. J. Wigham Richardson's Paper on the Modes of Estimating the Strains to which Steamers are subject.

FIG. 1.

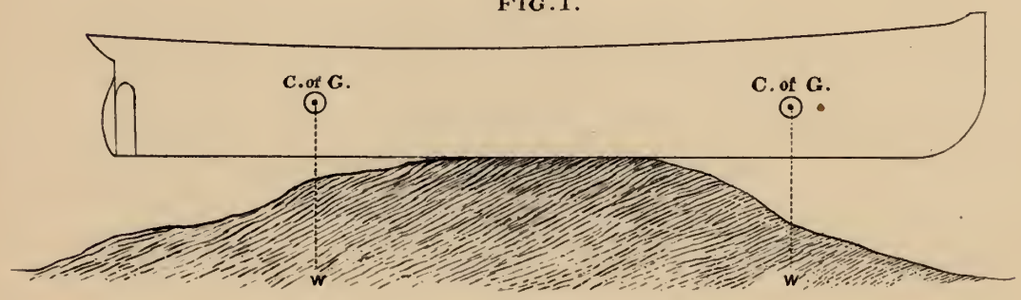


FIG. 2.

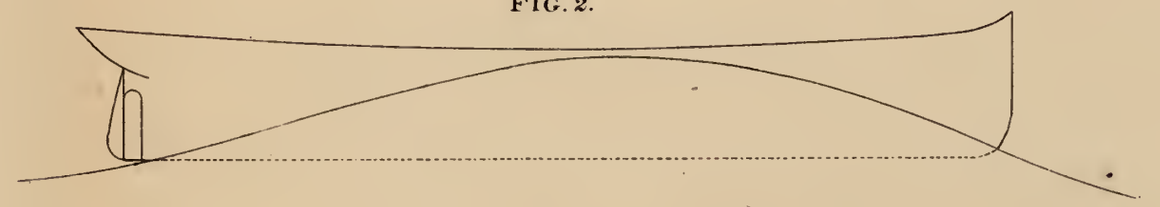


FIG. 3.

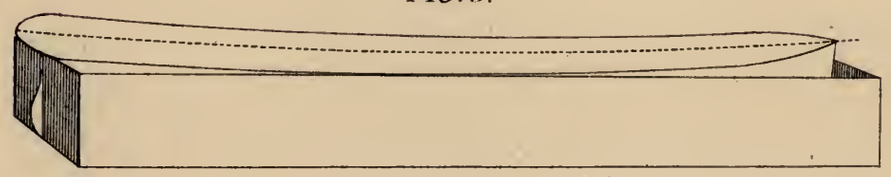


FIG. 4.

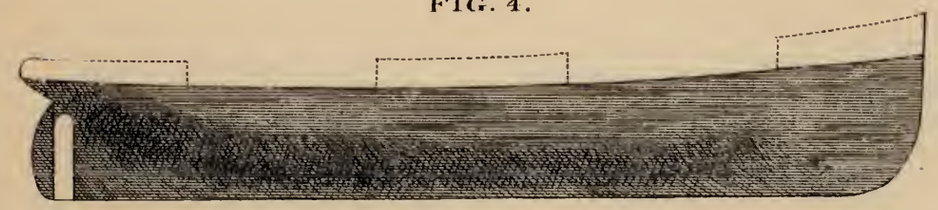


FIG. 5.

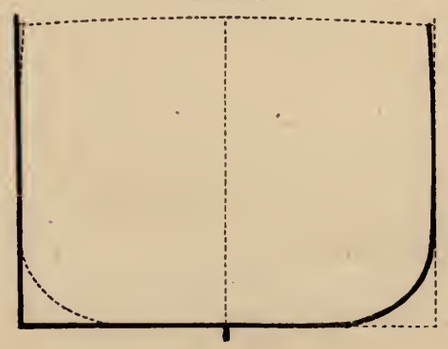
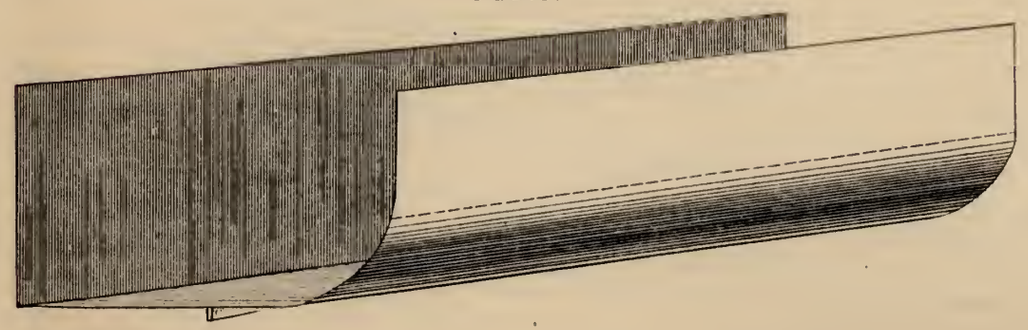
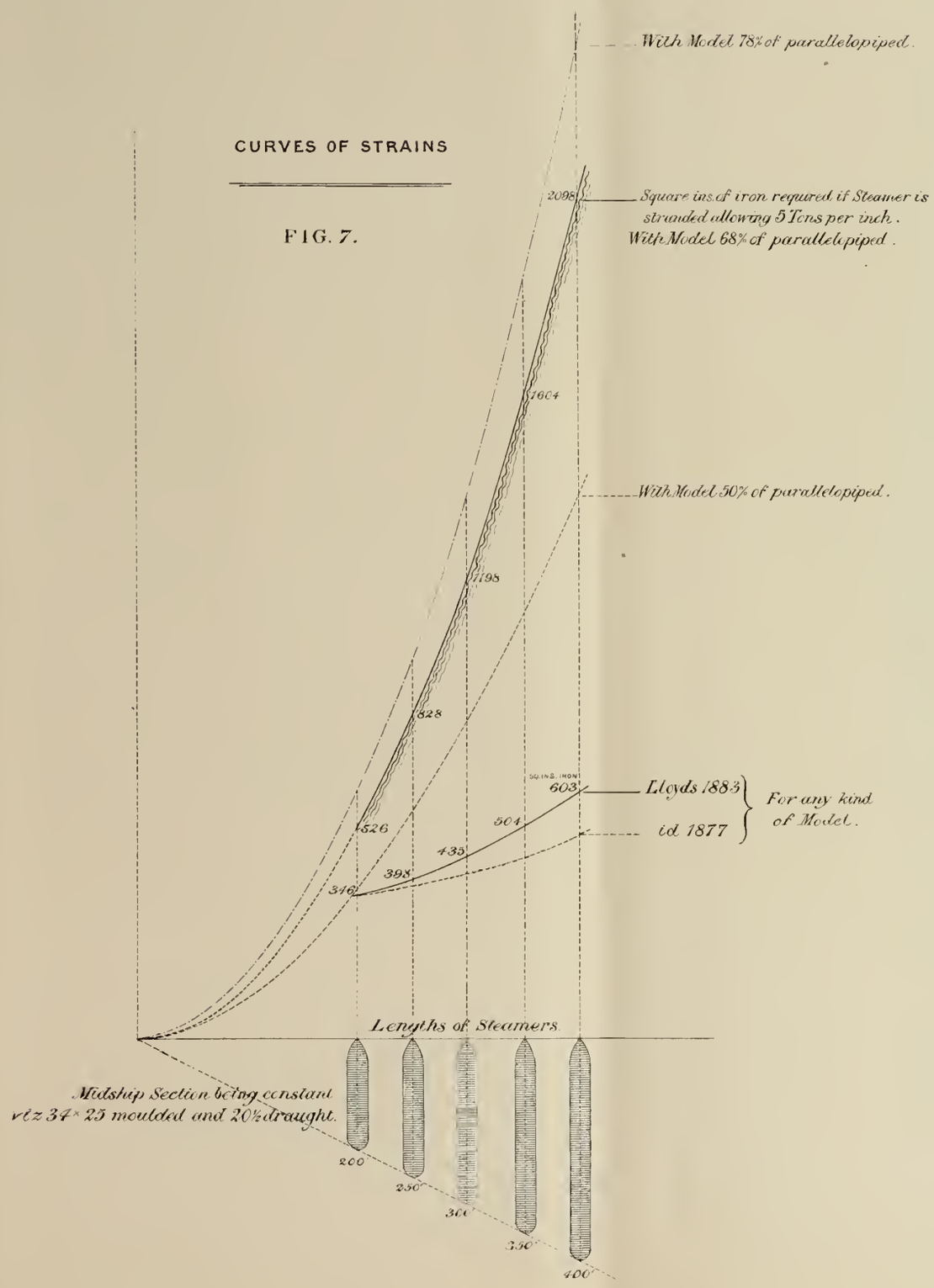


FIG. 6.



To Illustrate Mr. J. Wigham Richardson's Paper on the Modes of Estimating the Strains to which Steamers are subject



Trans. Inst. Naval Architects, Vol. XXIV., 1883.

To Illustrate Mr. V

FIG. 7.

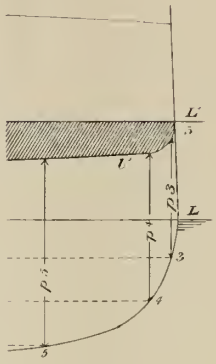
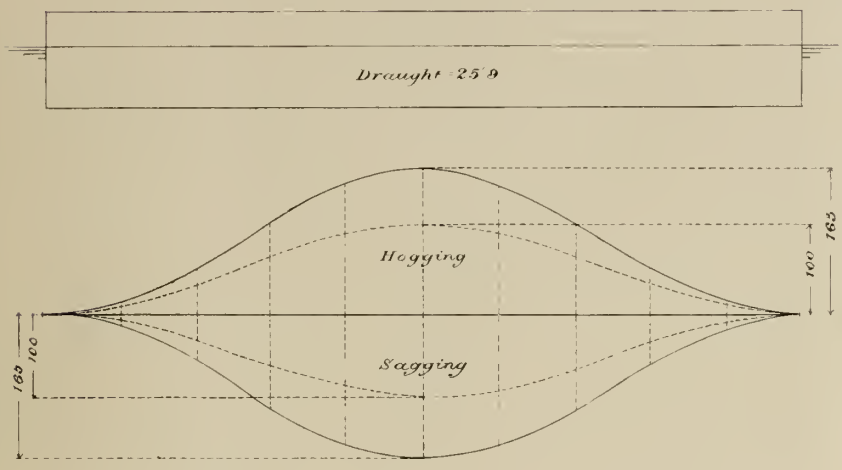


FIG. 1.



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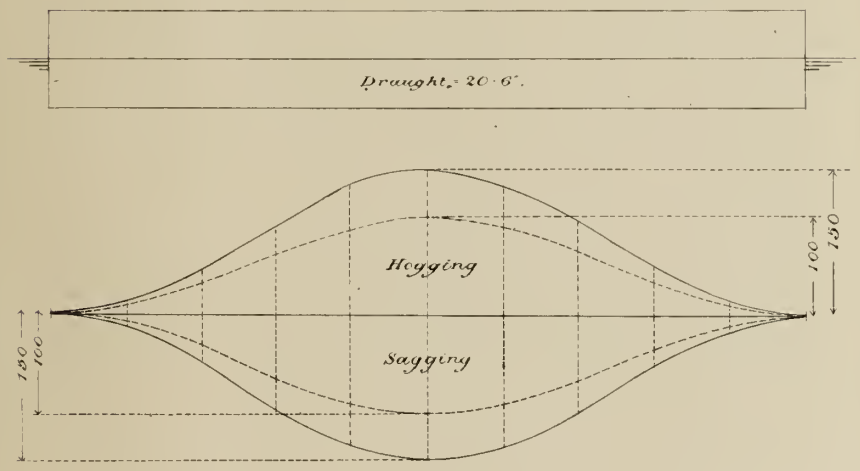
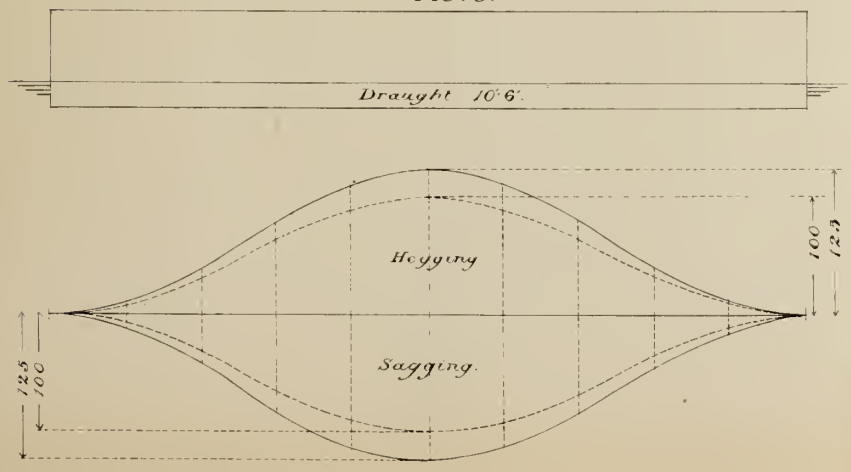


FIG. 3.



To Illustrate Mr. W. E. Smith's Paper on Hogging and Sagging Strains in a Seaway as Influenced by Wave Structures.

FIG. 1.

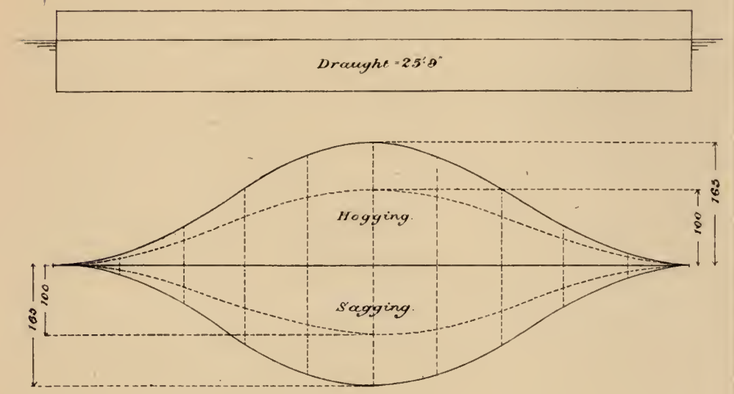


FIG. 2.

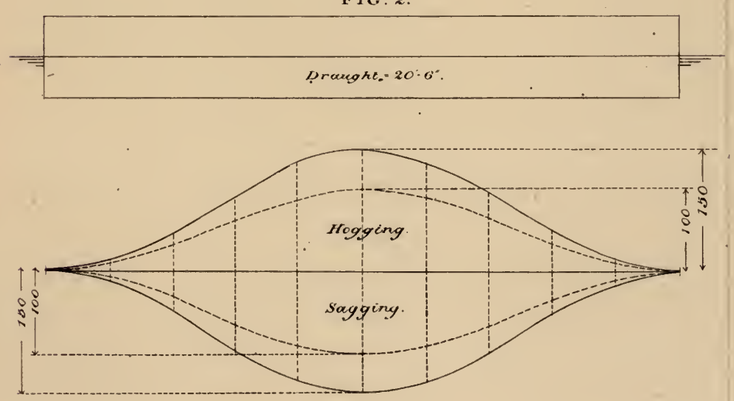


FIG. 3.

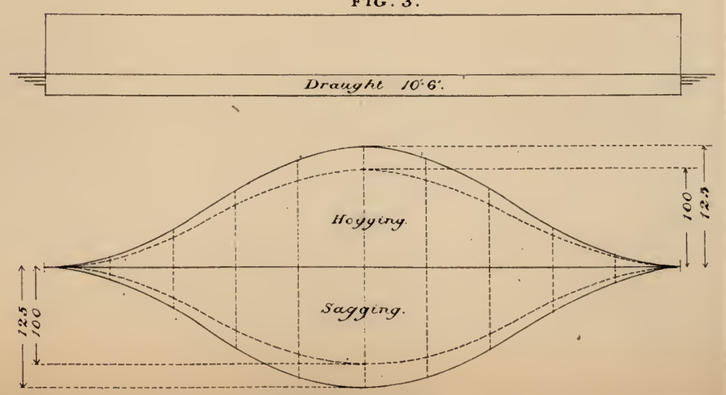


FIG. 4.

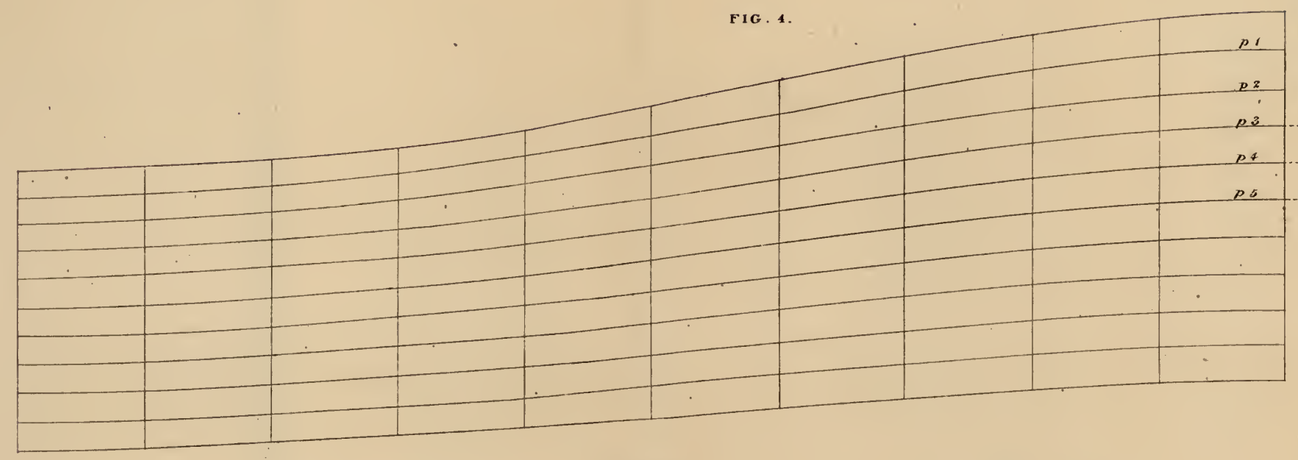


FIG. 7.

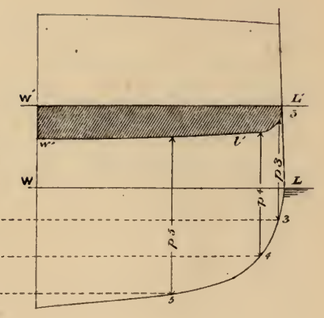


FIG. 5.

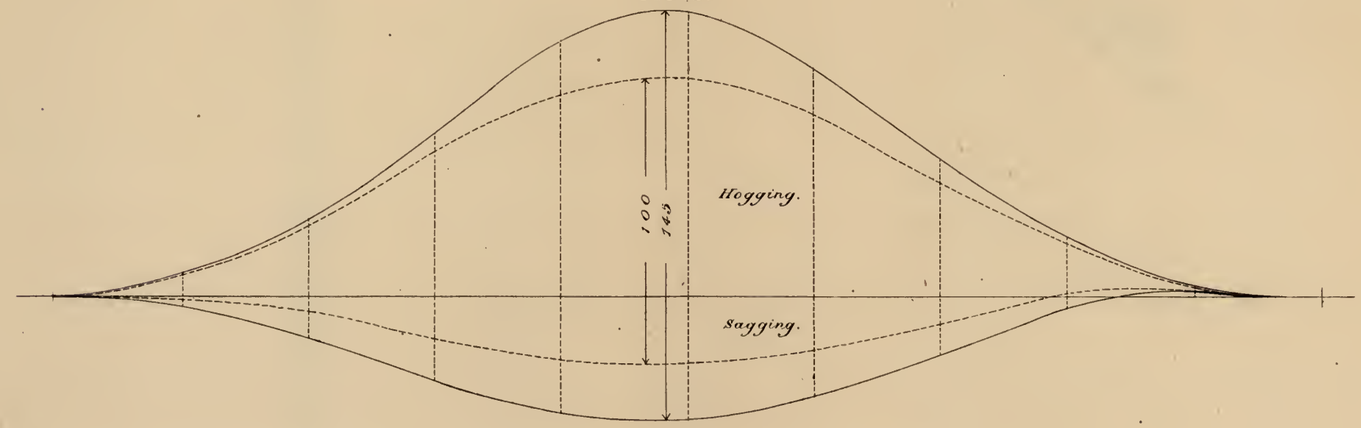
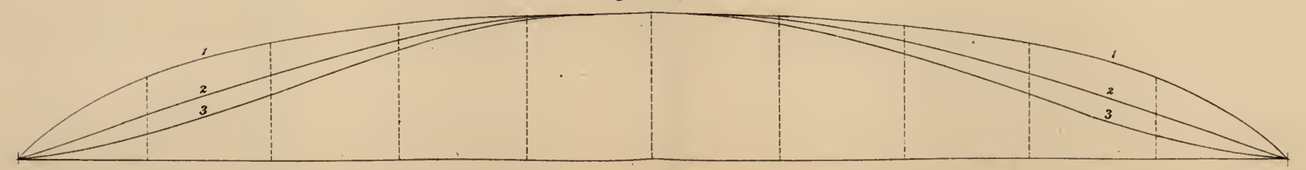


FIG. 6.



To Illustrate Mr. J

FIG. 1.

METACENTRIC CURVES OF STEAMERS
A, B, C, D & E.

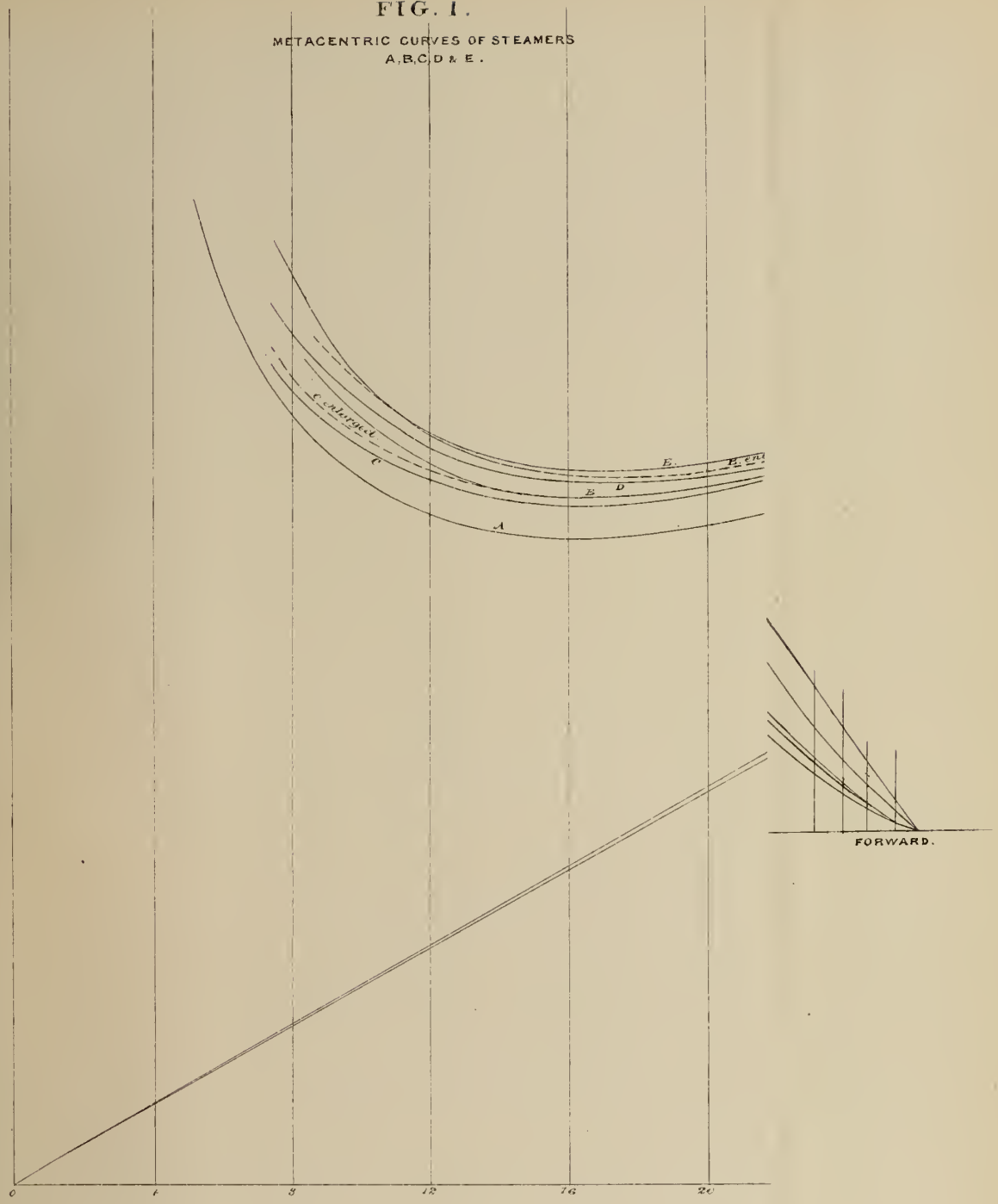
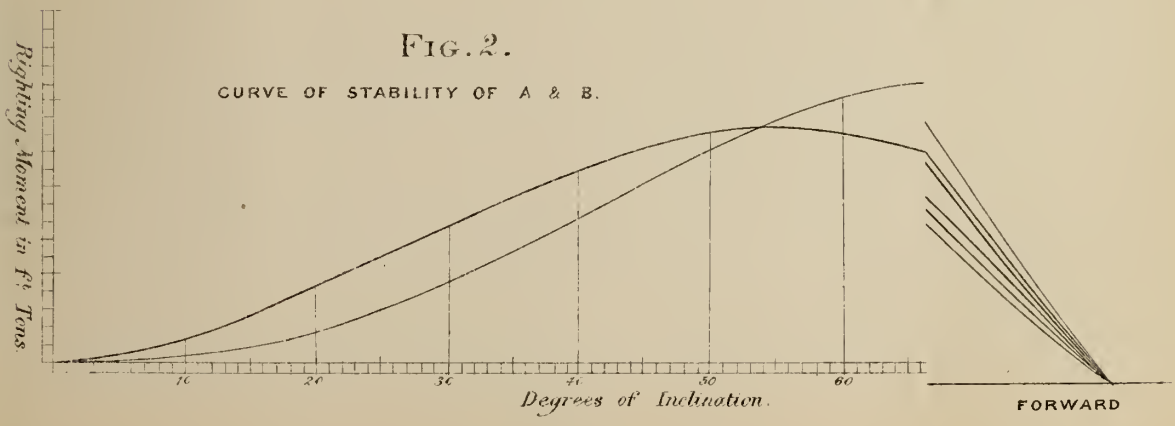


FIG. 2.

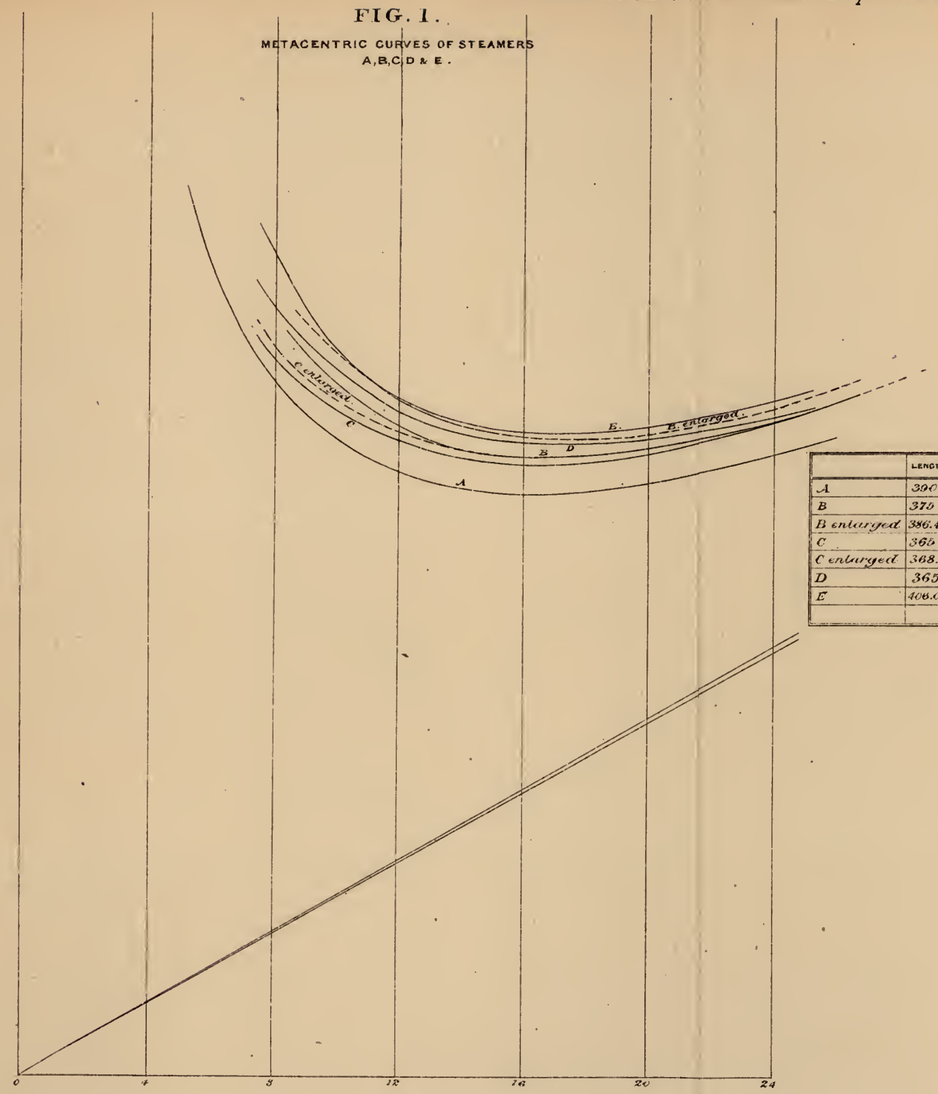
CURVE OF STABILITY OF A & B.



To Illustrate Mr. J. H. Biles' Paper on the Advantages of Increased Proportion of Beam to Length in Steamships.

FIG. 1.

METACENTRIC CURVES OF STEAMERS A, B, C, D & E.



	LENGTH	BREADTH
A	390	42
B	375	45
B enlarged	386.47	46.38
C	365	43
C enlarged	368.1	43.365
D	365	45'6"
E	406.04	48.46

FIG. 2.

CURVE OF STABILITY OF A & B.

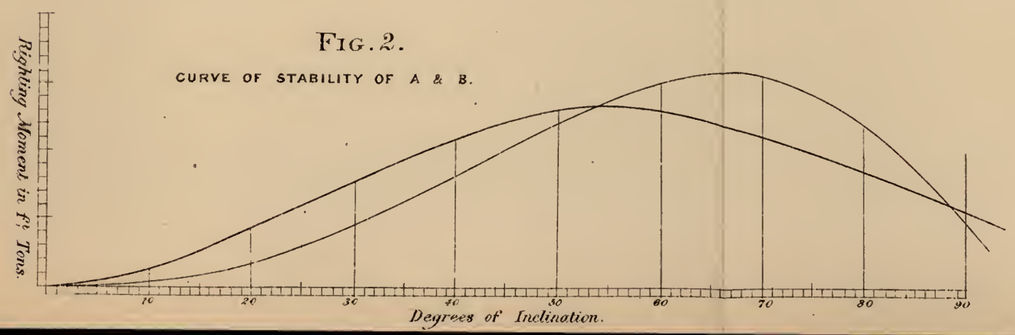


FIG. 4.

CURVES, SHEWING LONGITUDINAL DISTRIBUTION OF BUOYANCY OF STEAMERS A, B, C & D.

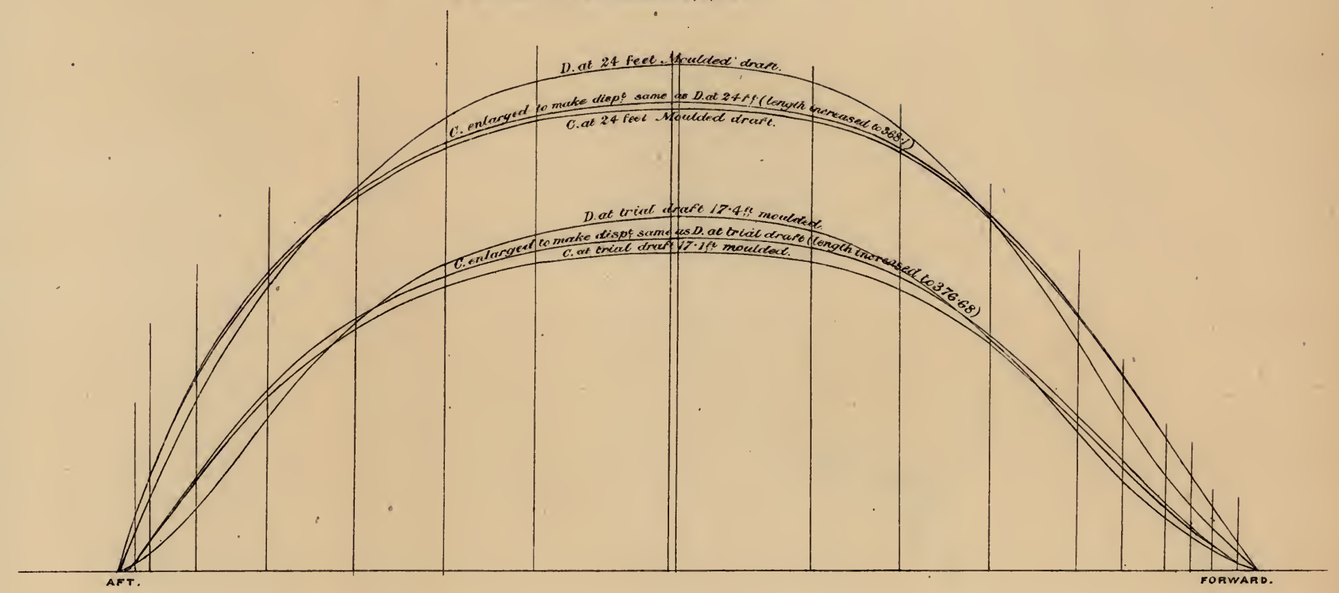
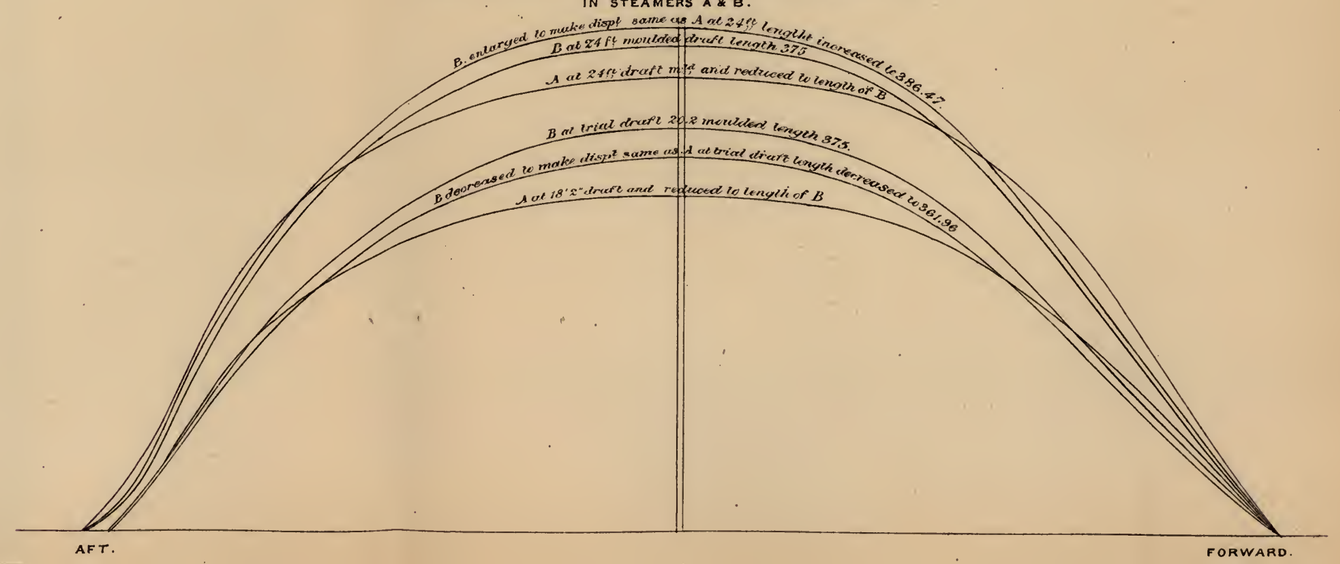


FIG. 3.

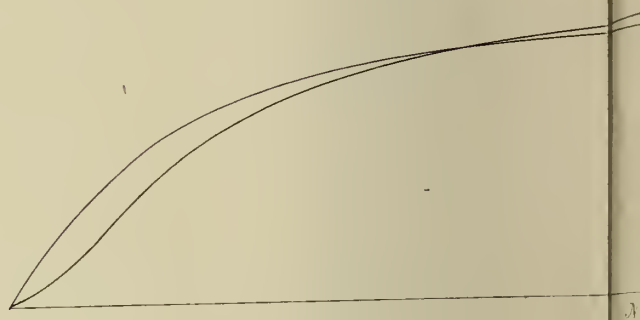
CURVES SHEWING LONGITUDINAL DISTRIBUTION OF BUOYANCY. IN STEAMERS A & B.



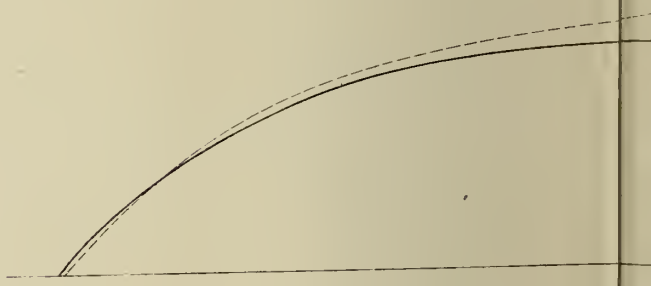
Trans. Inst. Naval Architects, Vol. XXIV., 1883.

To Illustrate Mr. J. H. Biles' Paper on the Advantag

CURVES OF INT
A & B. A.



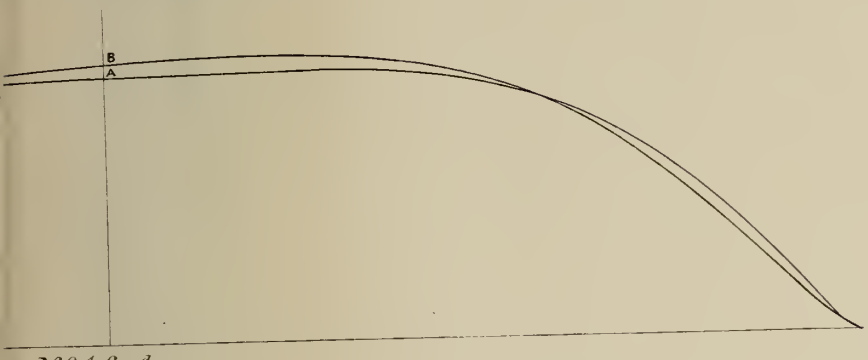
INTERNAL CA



of Increased Proportion of Beam to Length in Steamships.

FIG. 5.

NAL CAPACITY OF STEAMERS
DUCED TO LENGTH OF B.



No. 1 for.d

FIG. 6.

ACITY CURVES OF STEAMERS C & D.

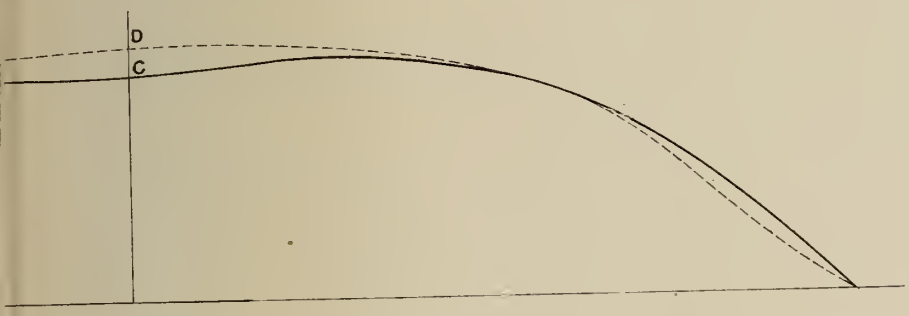


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FIG. I.

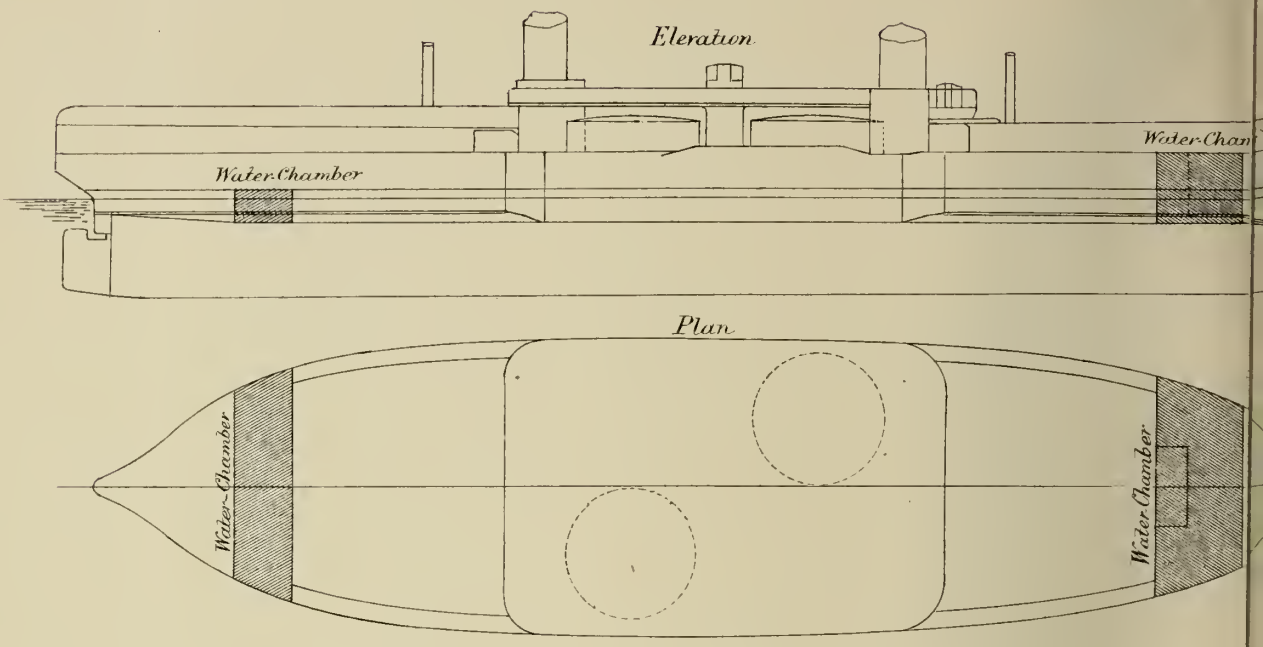
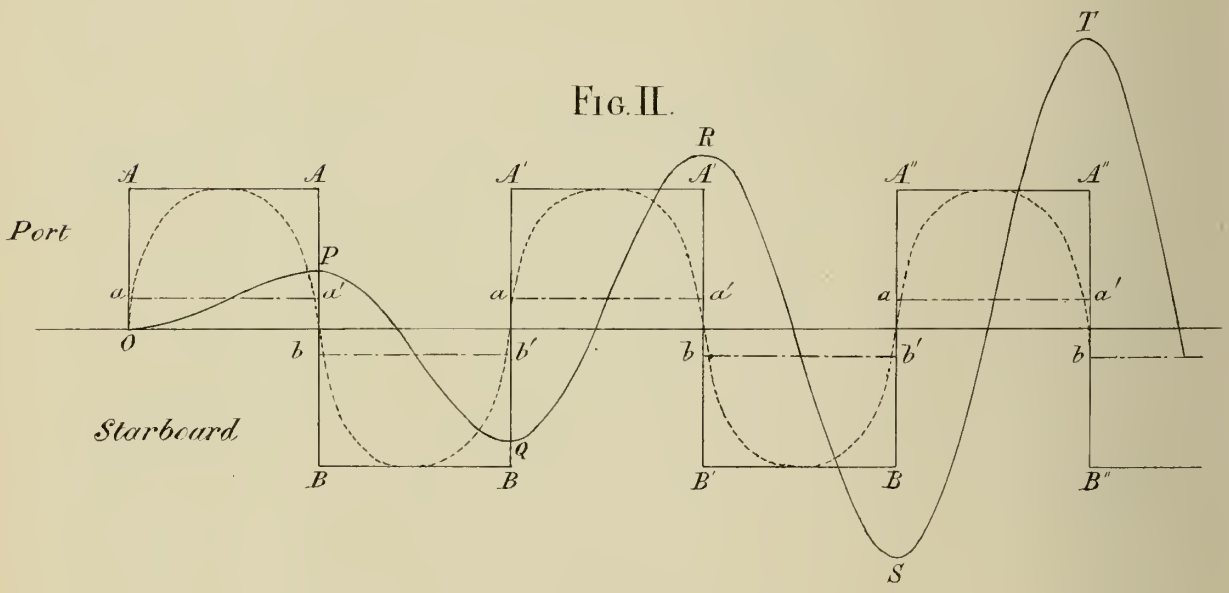
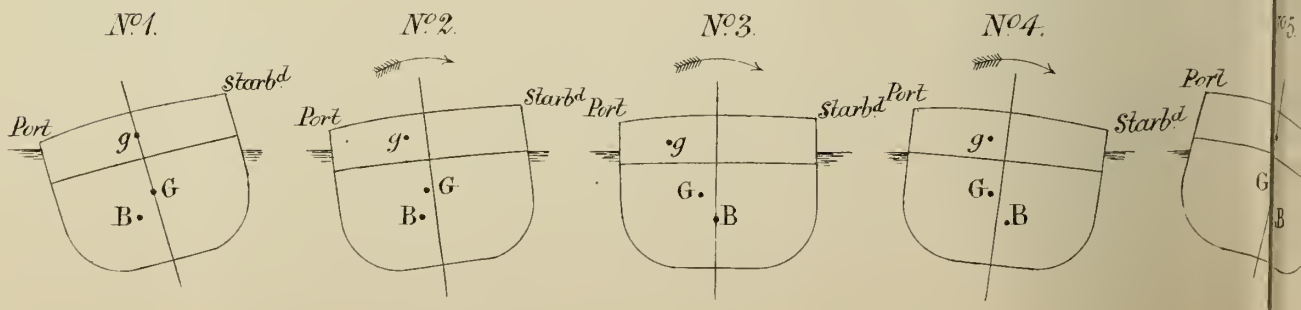


FIG. II.





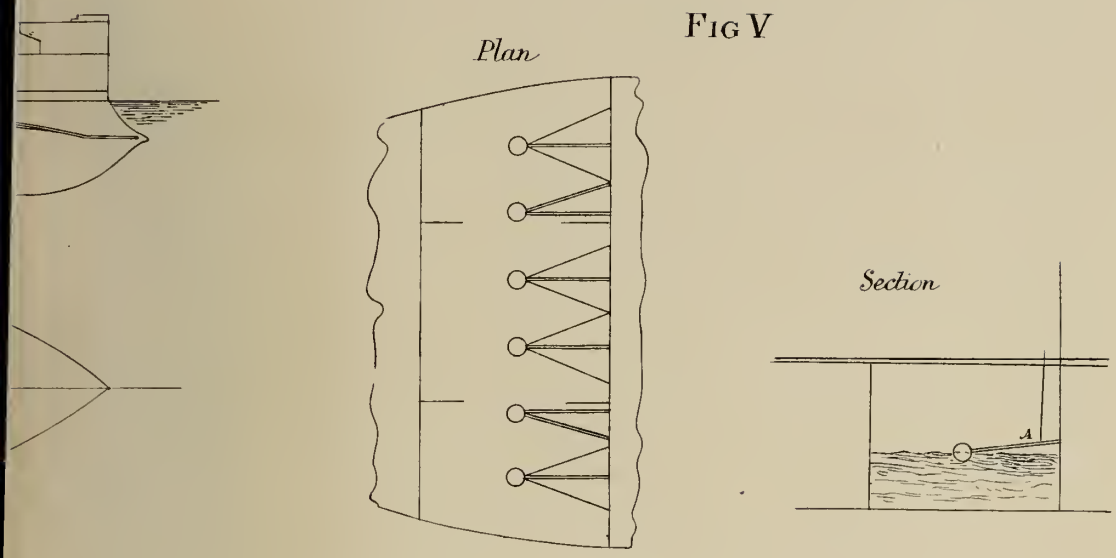


Fig V
Plan
Section
A

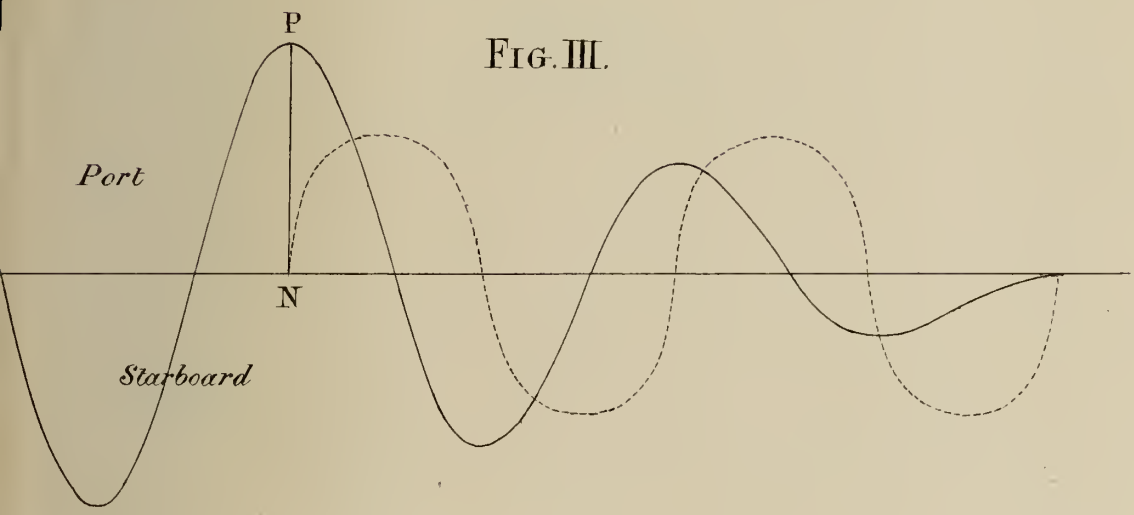
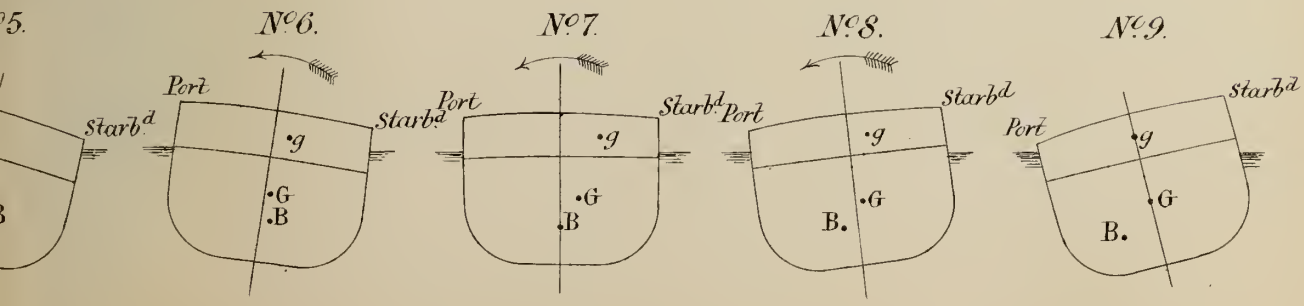


Fig. III.
P
Port
N
Starboard


G. IV.
5.
No 6.
No 7.
No 8.
No 9.
Starb.d
Port
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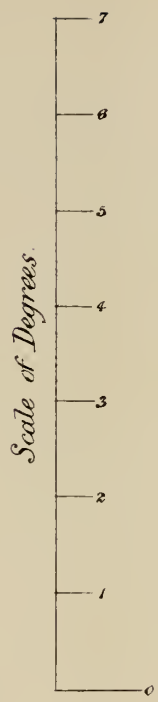
G
B


Scale of Degrees.
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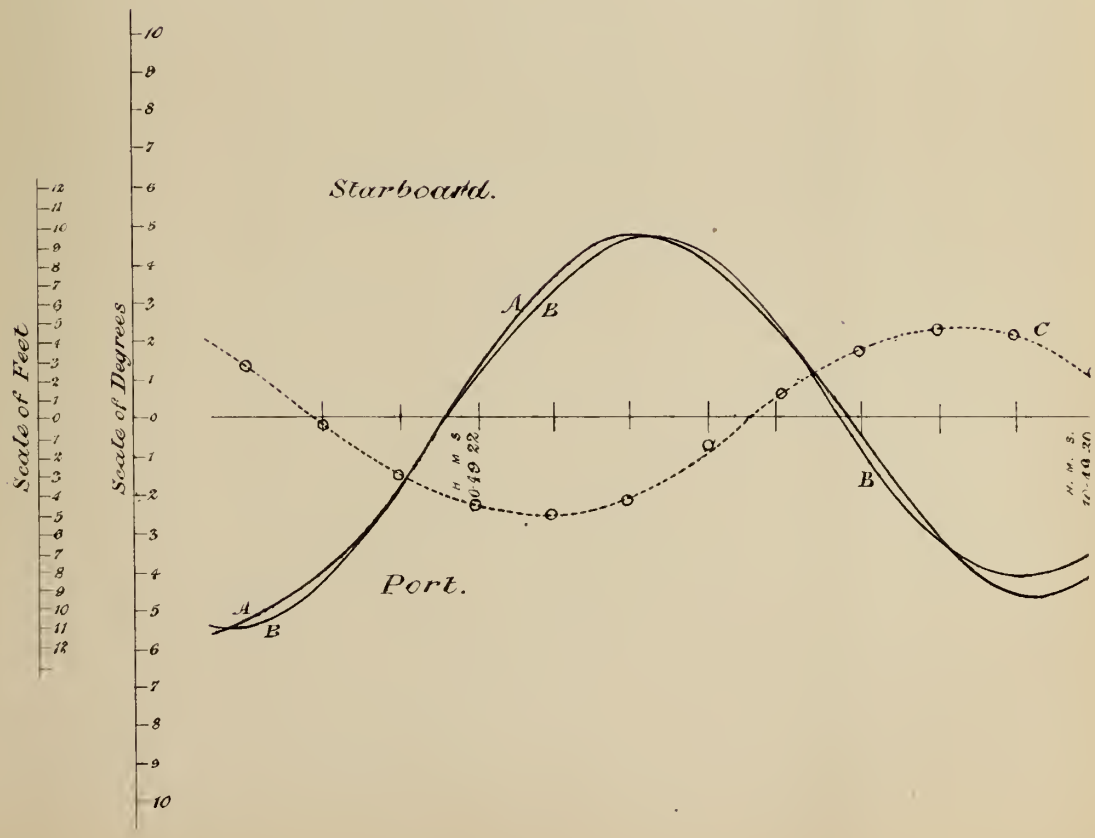
Scale of Feet
Scale of Degrees
Starboard.
Port.
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B
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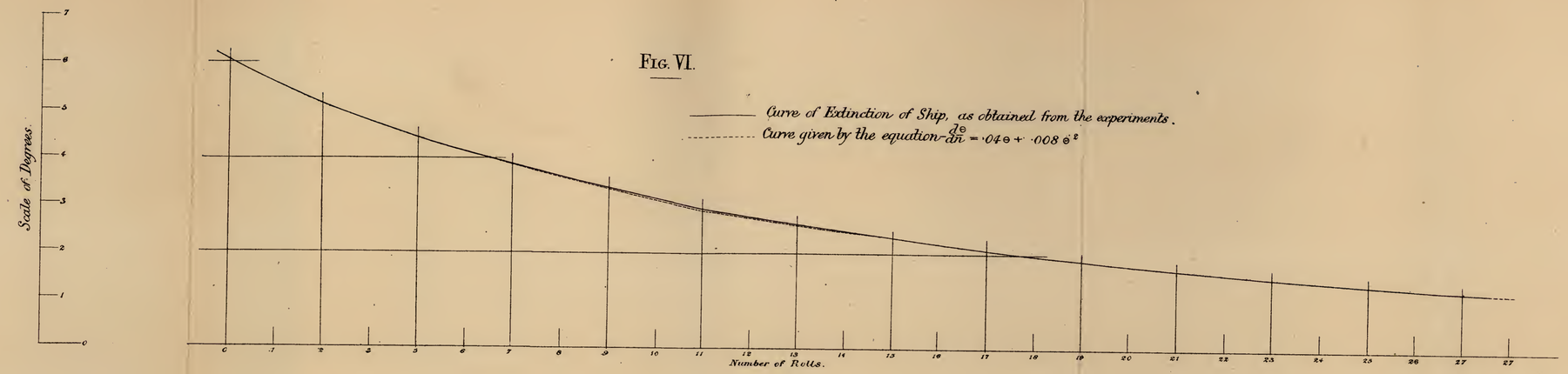
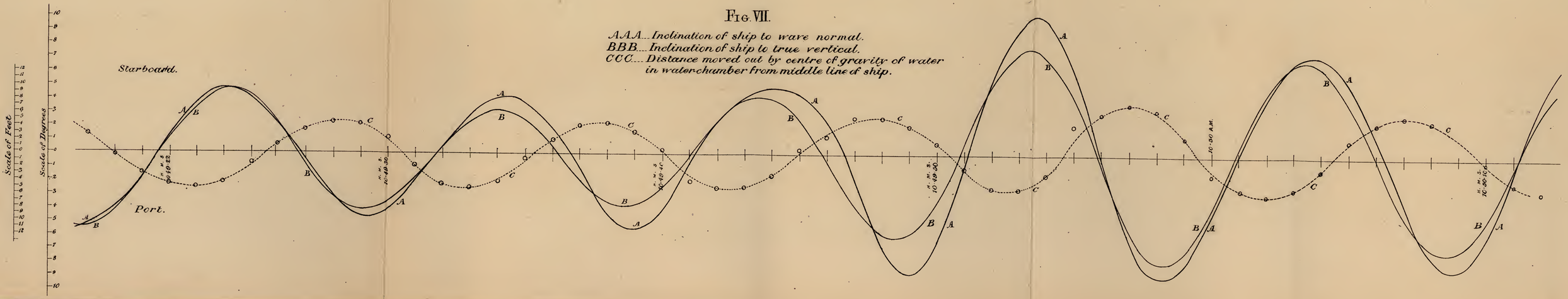
To Illustrate Mr. P. Watts' Paper on a Method of Reducing the Rolling of Ships at Sea.





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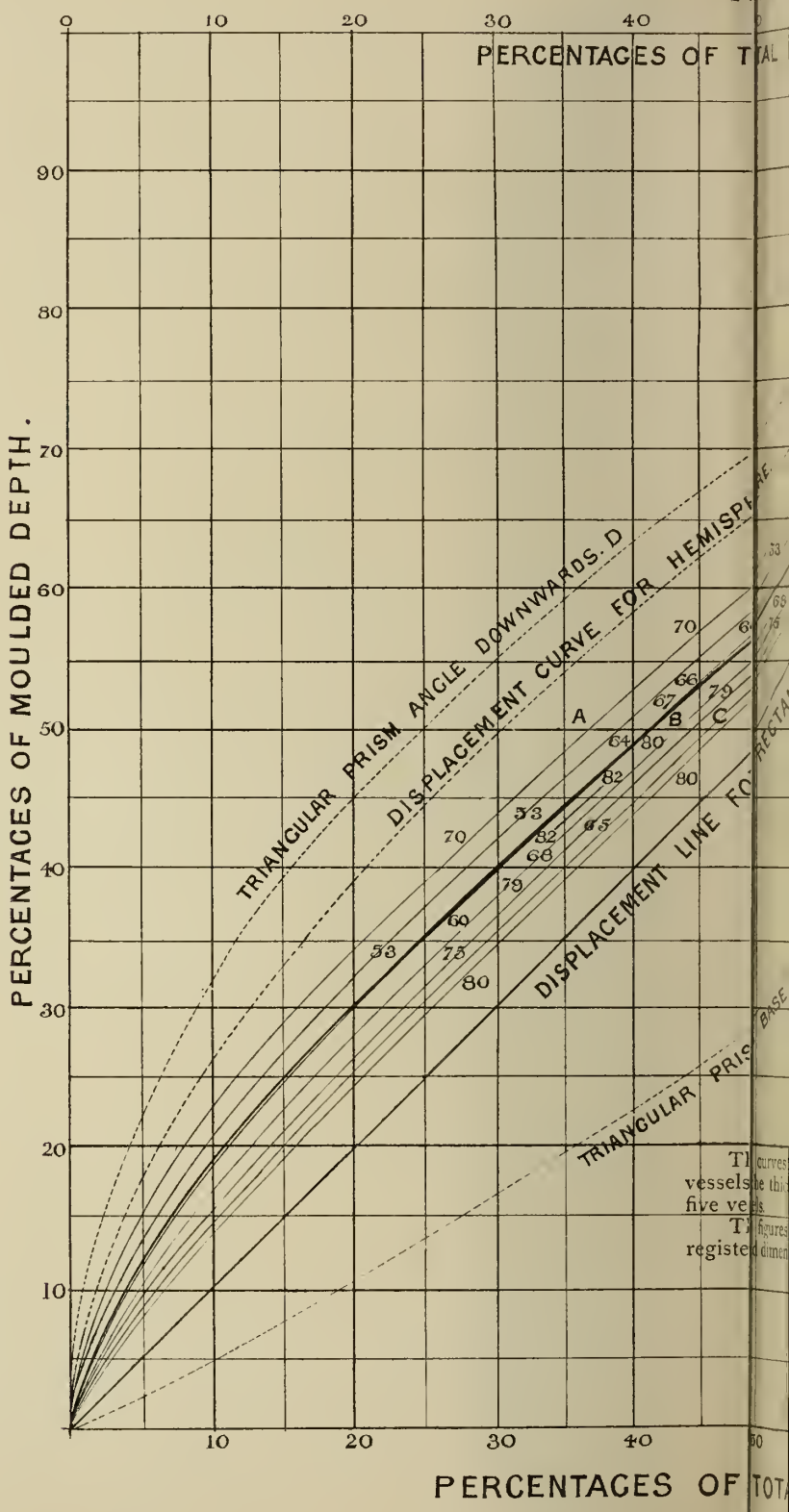
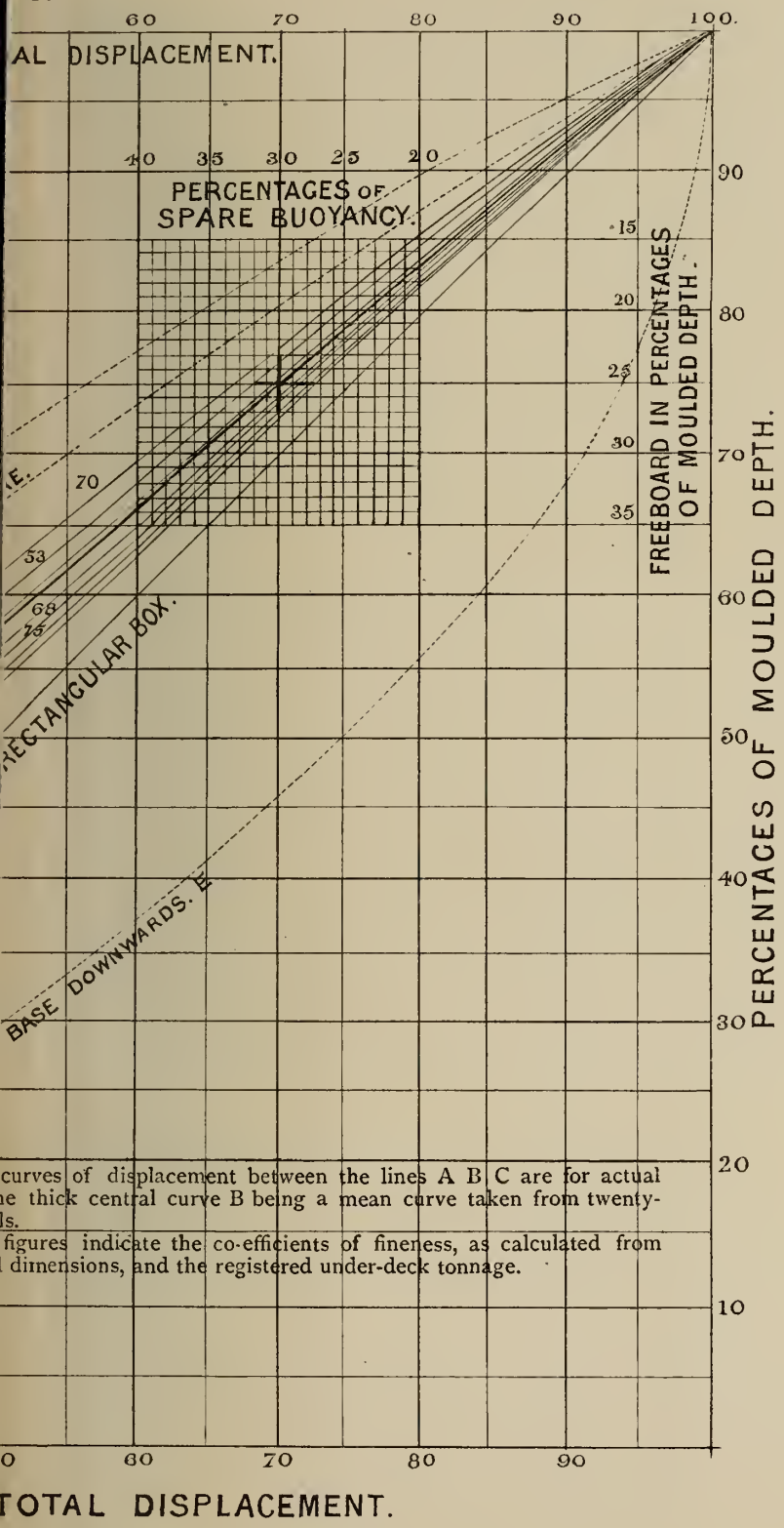
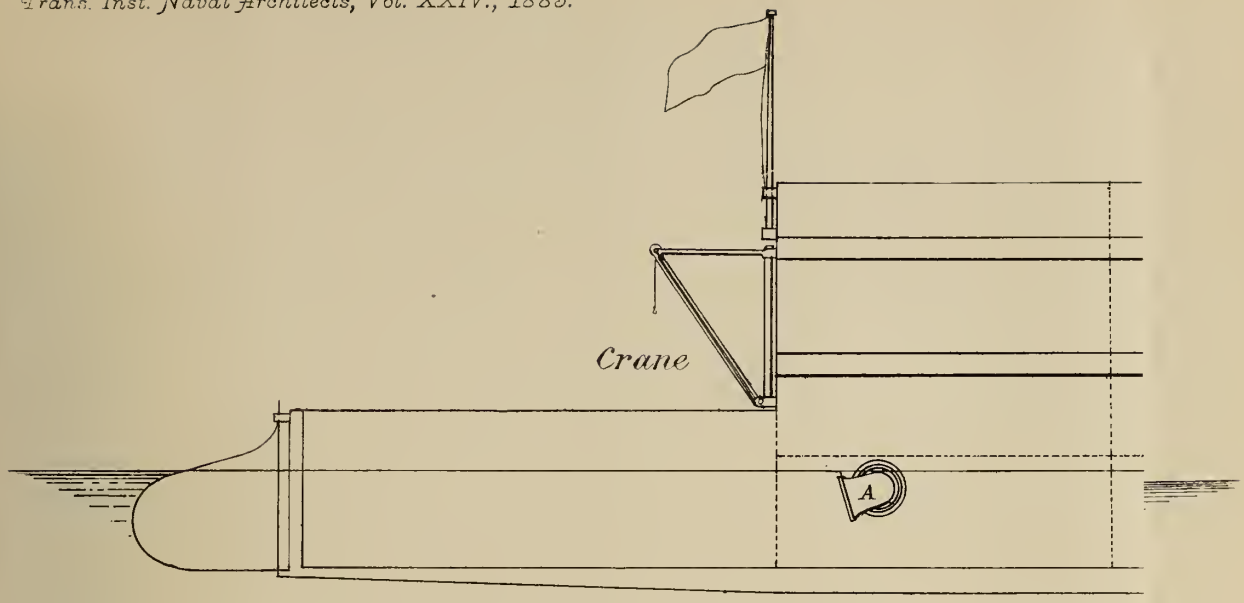
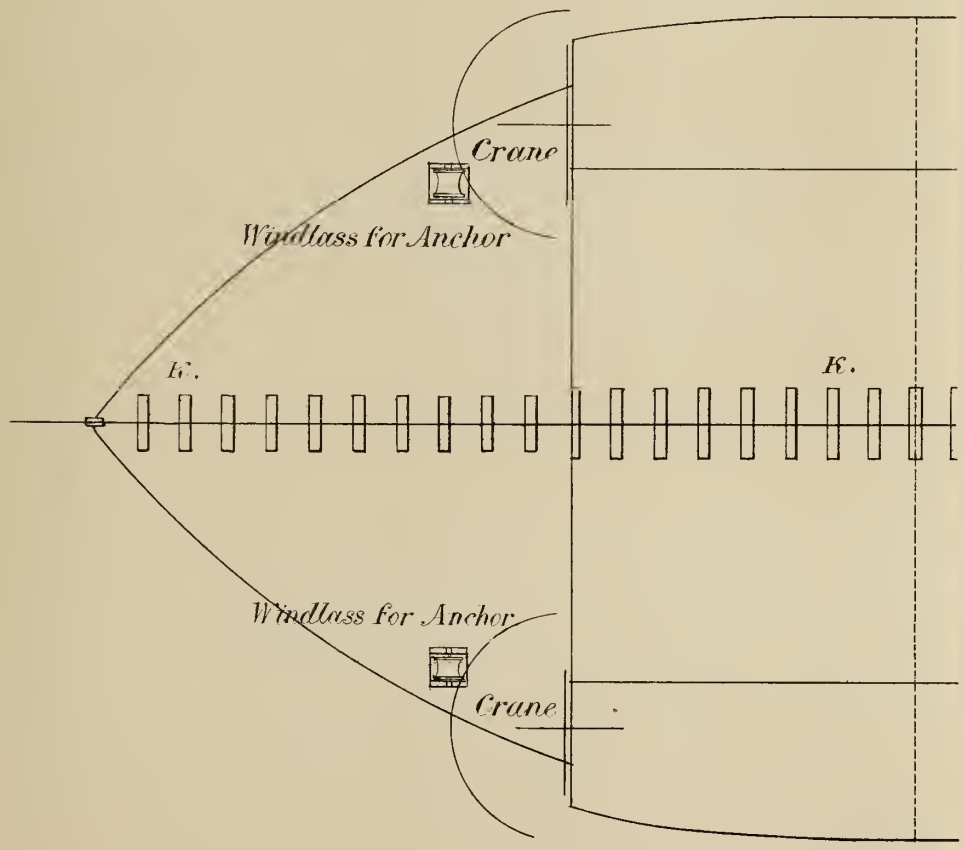


and the Official Register in Relation to the Freeboard of Iron Vessels.

1.

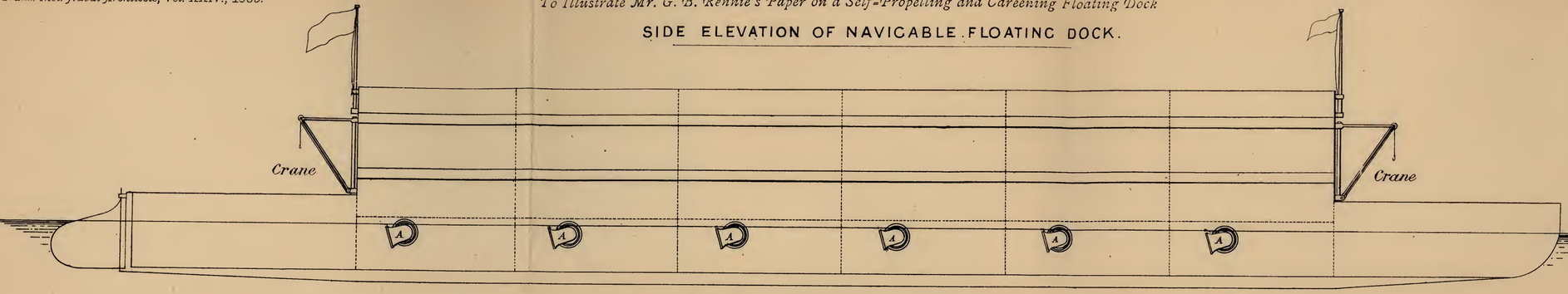



Crane
A

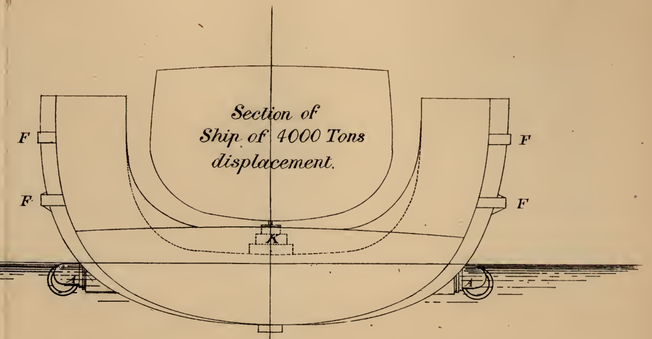

Crane
Windlass for Anchor
K.
K.
Windlass for Anchor
Crane

To Illustrate Mr. G. B. Rennie's Paper on a Self-Propelling and Careening Floating Dock

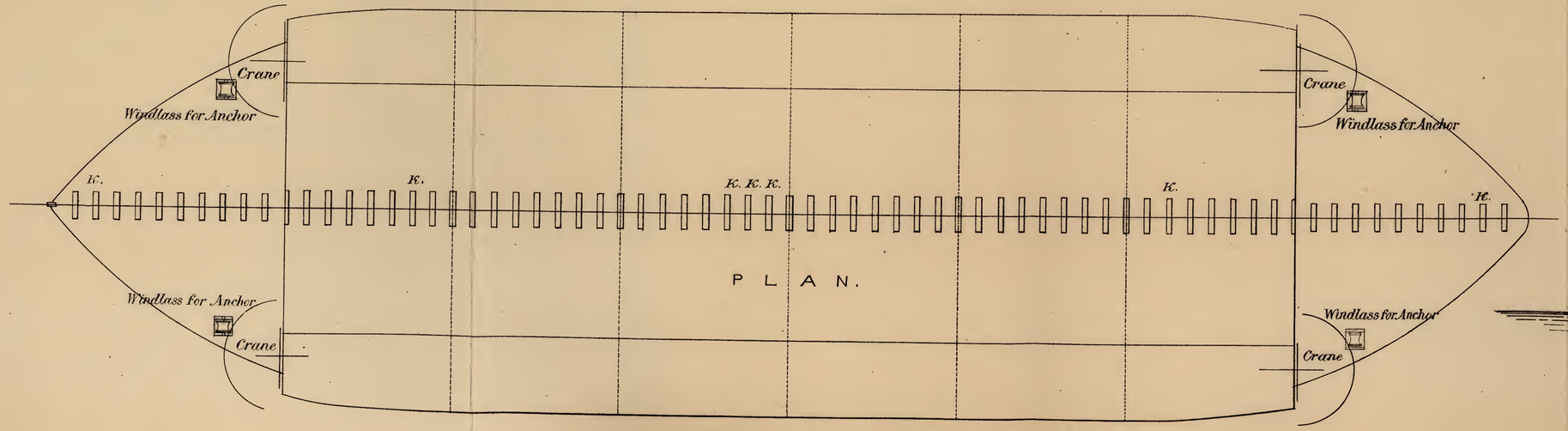
SIDE ELEVATION OF NAVICABLE FLOATING DOCK.

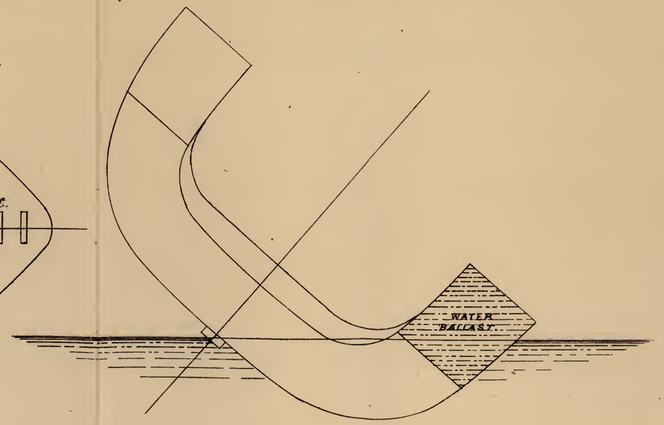


A. discharge Noxzles for propulsion
K. Keel Blocks in plan.



Cross Section, shewing Dock with section of Ship of 4000 Tons and 22 feet draught of water.
A. A. discharge Noxzles for propulsion
F. F. Wood Fenders.
K. Keel Blocks.





Section Shewing Dock when careened.

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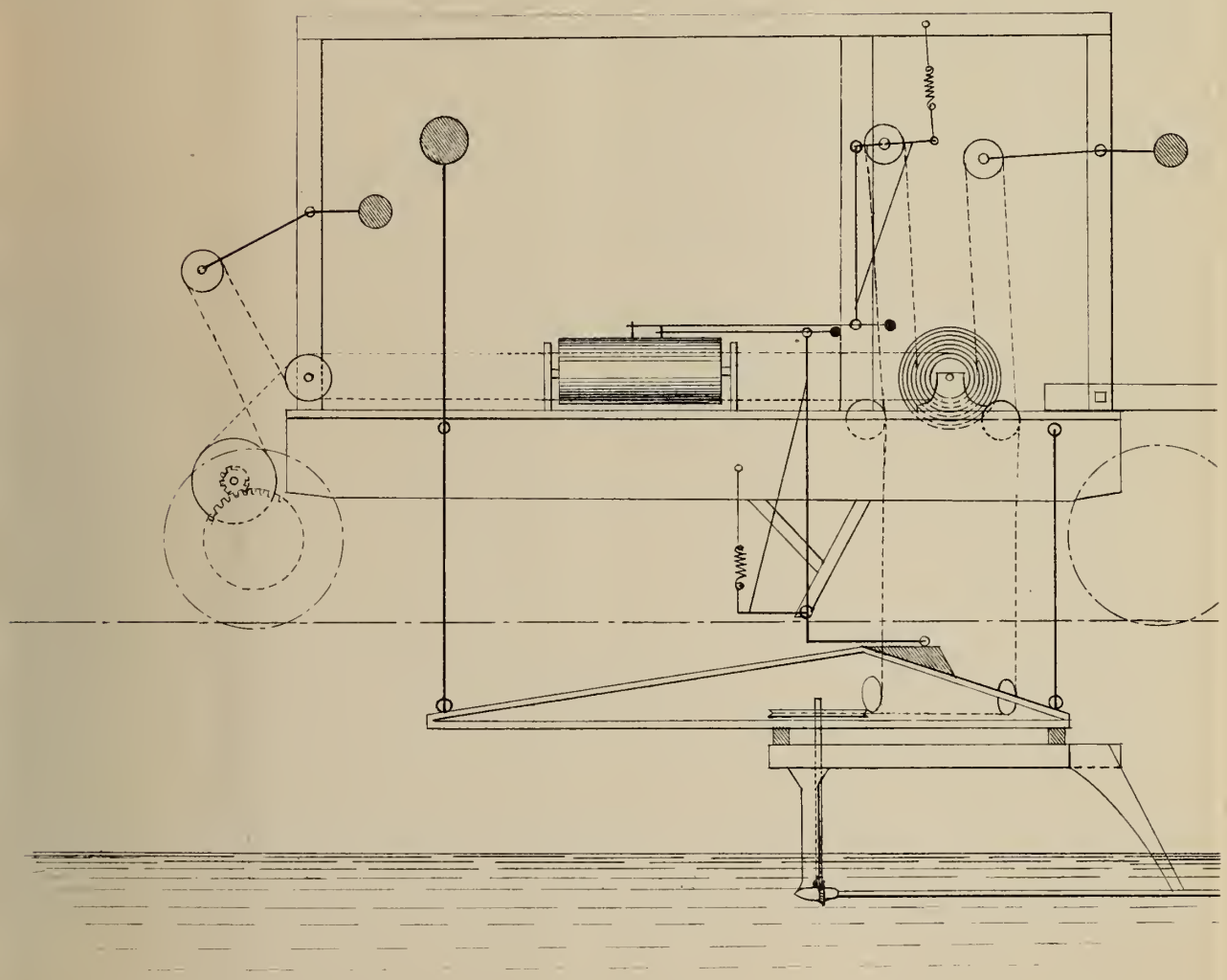


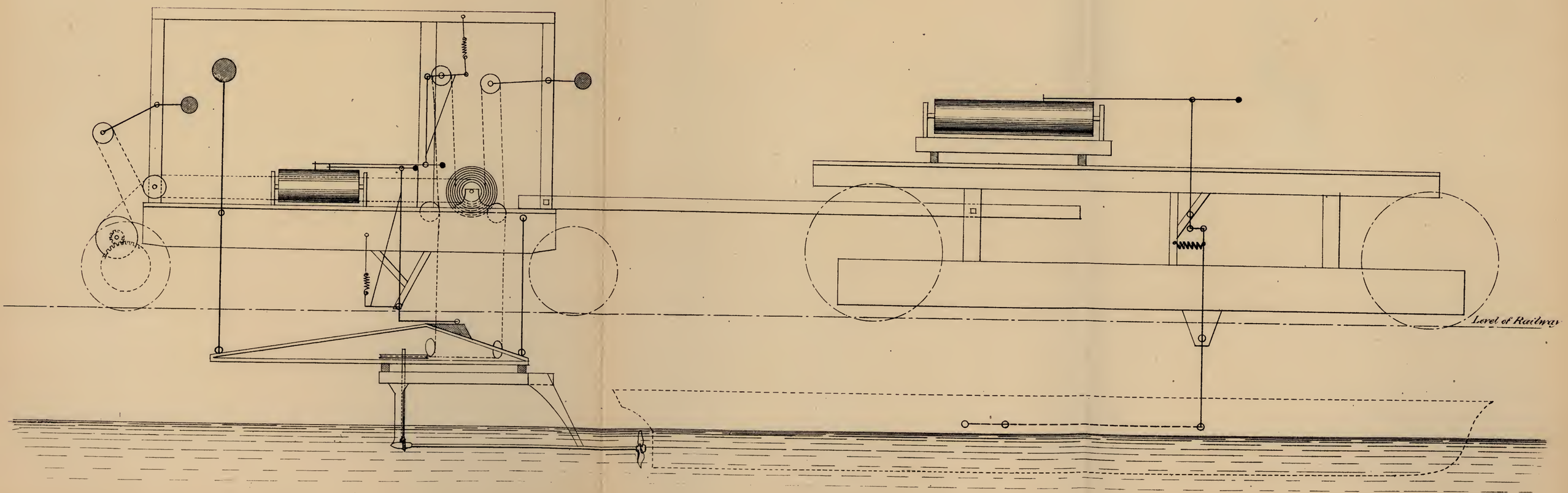
To Illustrate Mr. R. E. Froude's Paper on a Description of a Method of Investigation of Screw-Propeller Efficiency.

FIG. 1.

SHEWING ARRANGEMENT OF SCREW AND MODEL FOR EXPERIMENT.

Scale 1/16th full size.

NB. *The principal levers and other moving parts of the mechanism are shewn by skeleton lines. The arrangement of framing etc. as shewn, is also in a great measure figurative, to simplify the representation.*



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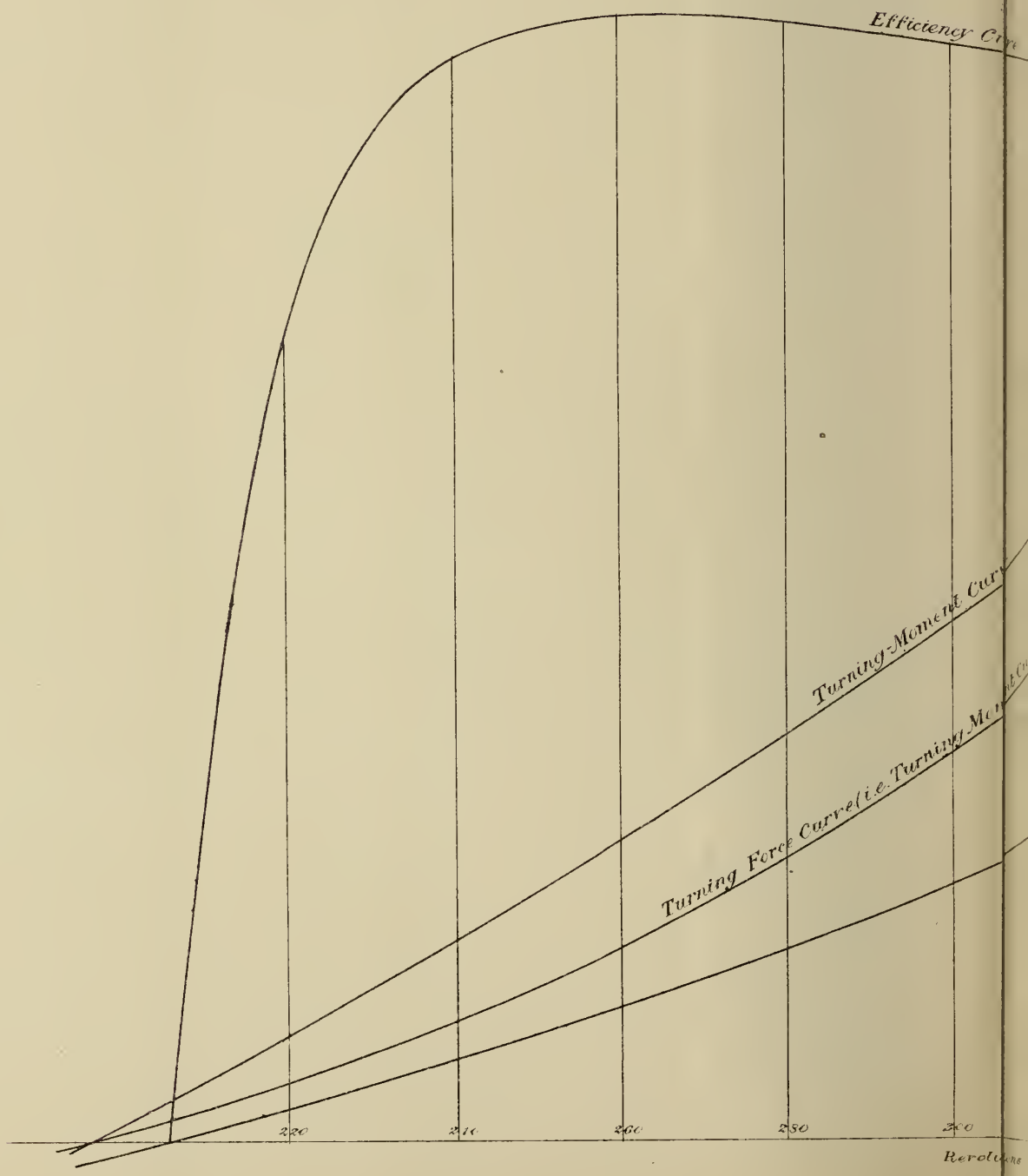
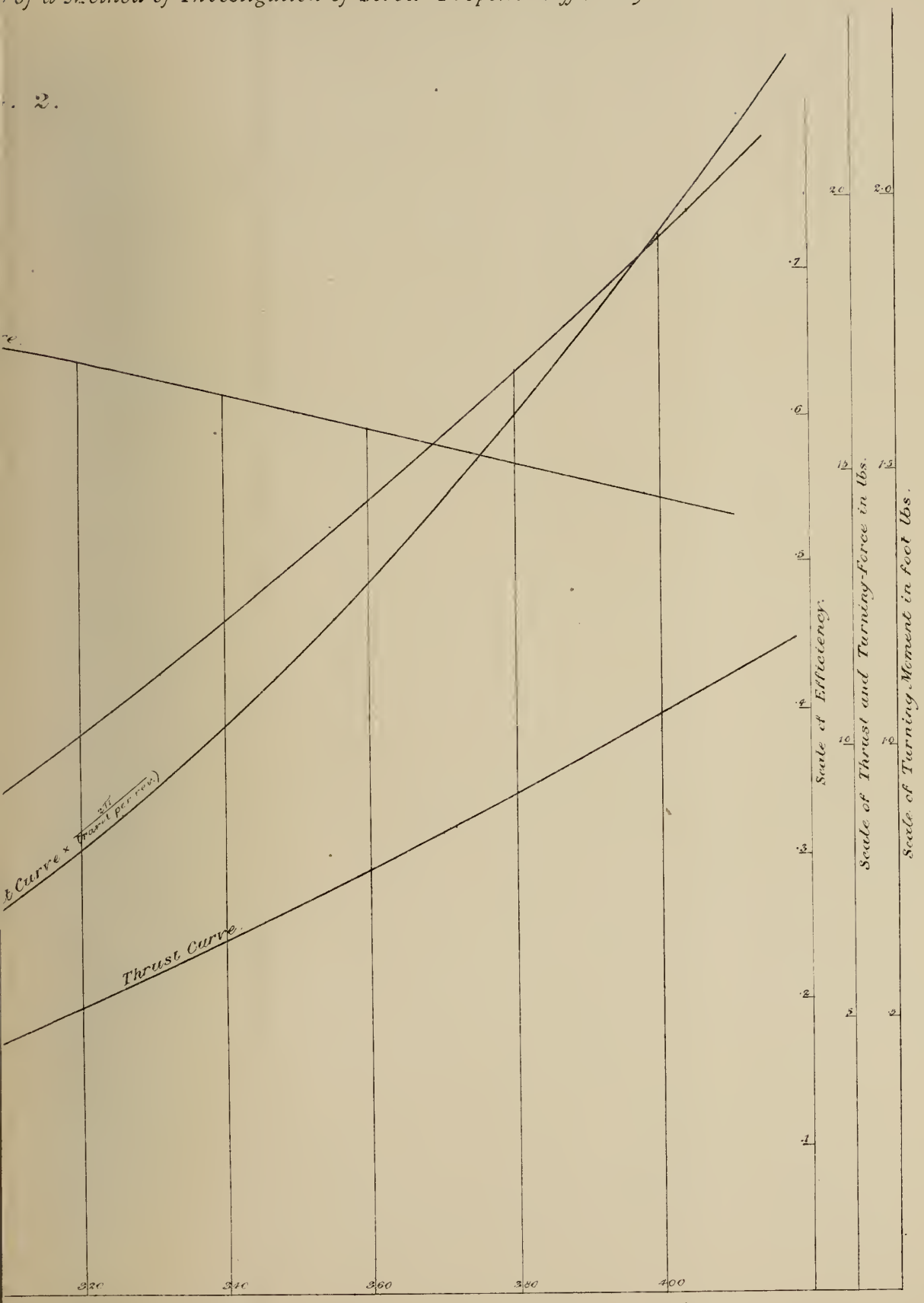
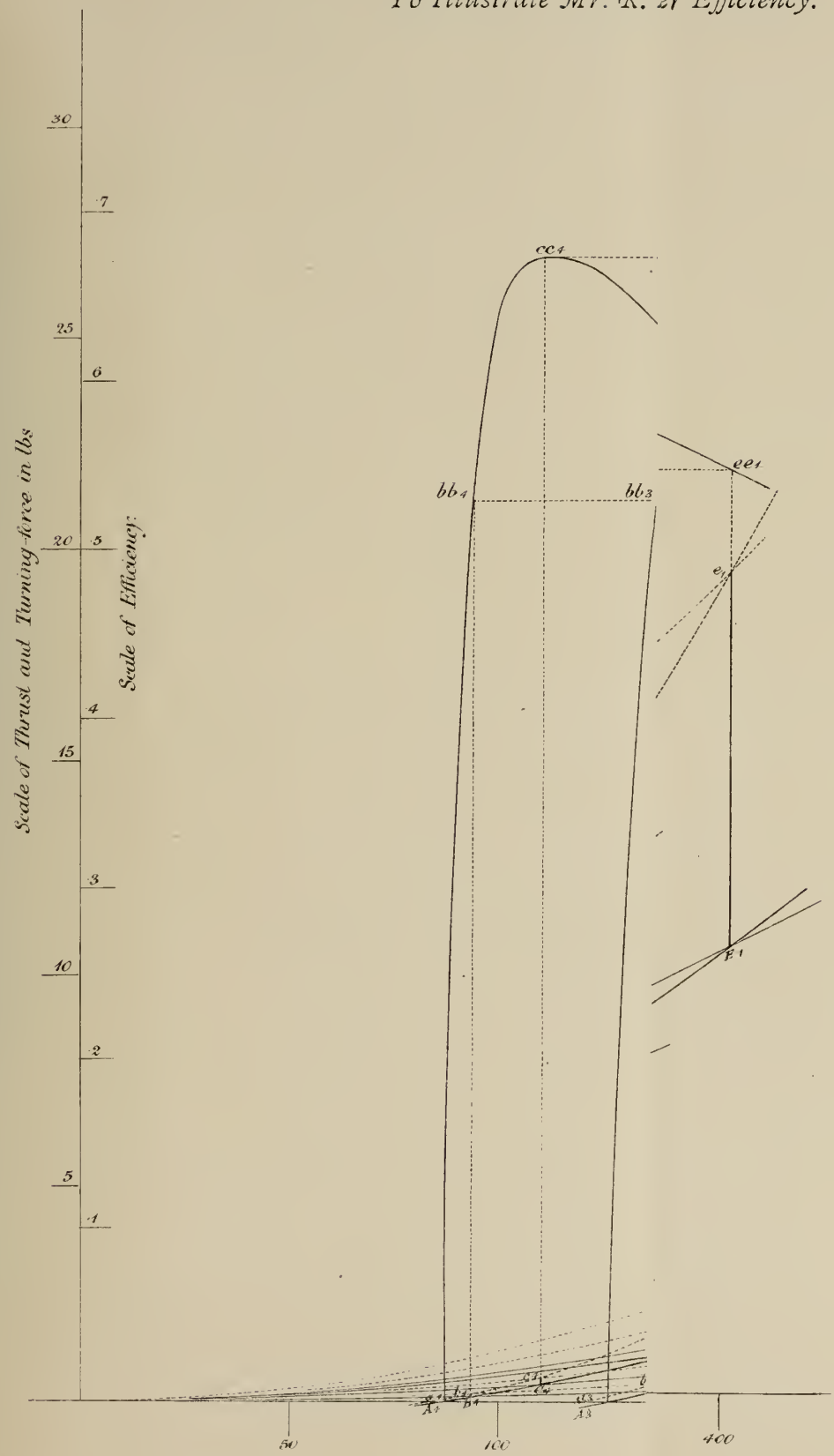


: of a Method of Investigation of Screw-Propeller Efficiency.

. 2.

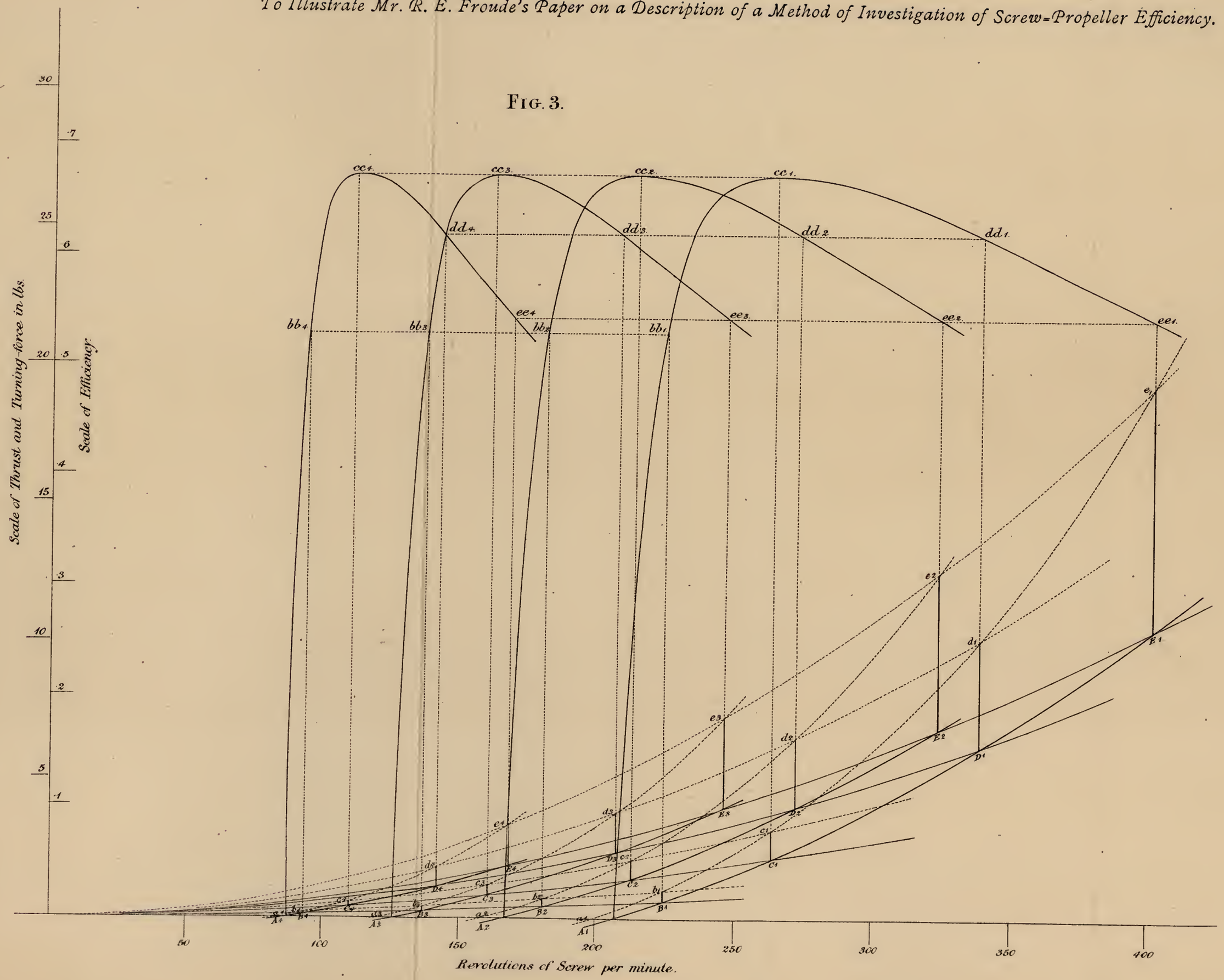


To Illustrate Mr. R. 2r Efficiency.



To Illustrate Mr. R. E. Froude's Paper on a Description of a Method of Investigation of Screw-Propeller Efficiency.

FIG. 3.



To Illustrate Mr. R. E. Froude's Paper on a Description of a Method of Investigation of Screw-Propeller Efficiency.

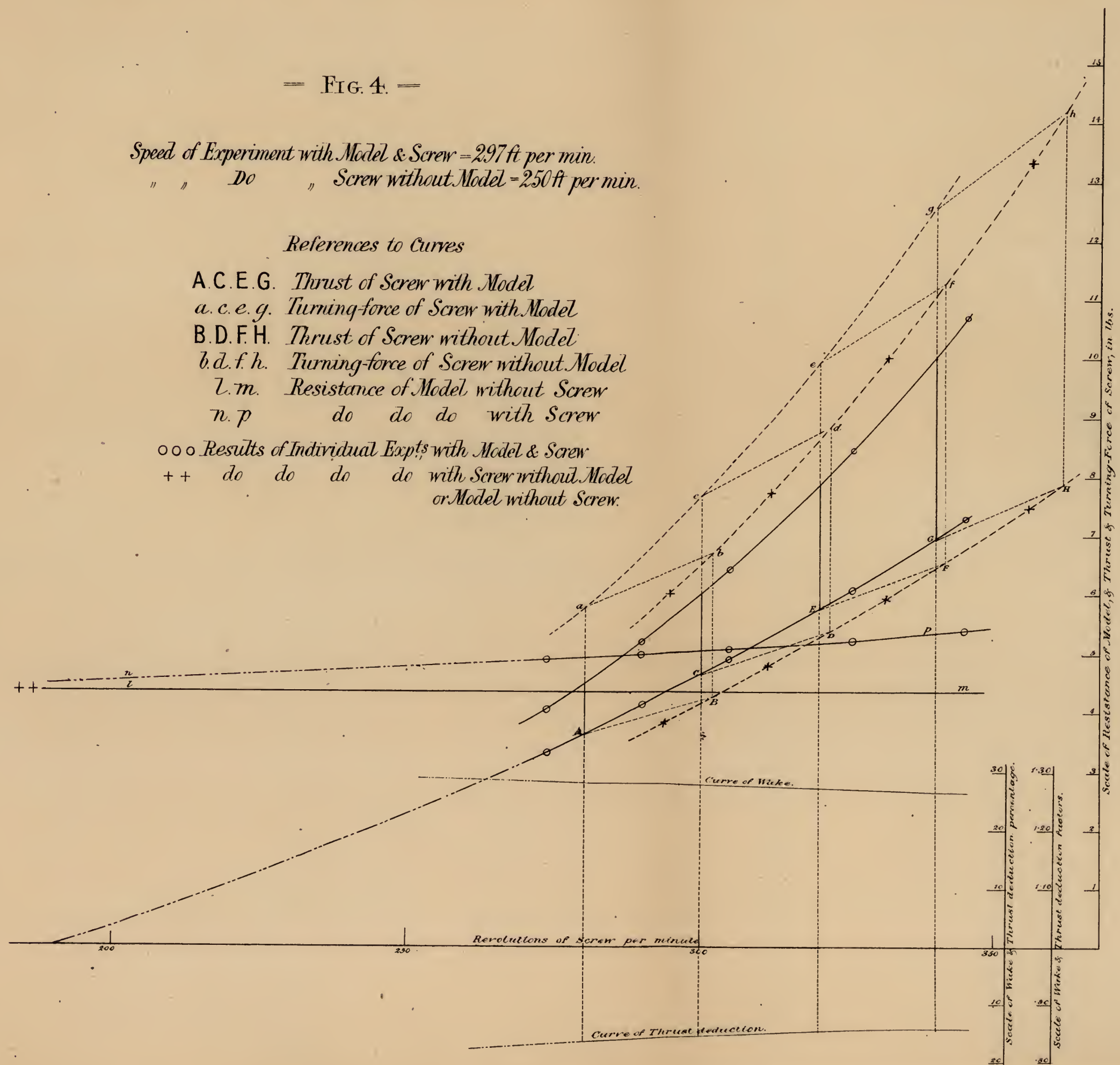
= FIG. 4. =

Speed of Experiment with Model & Screw = 297 ft per min.
" " Do " Screw without Model = 250 ft per min.

References to Curves

A.C.E.G. *Thrust of Screw with Model*
a.c.e.g. *Turning-force of Screw with Model*
B.D.F.H. *Thrust of Screw without Model*
b.d.f.h. *Turning-force of Screw without Model*
l.m. *Resistance of Model without Screw*
n.p *do do do with Screw*

ooo *Results of Individual Expts with Model & Screw*
++ *do do do do with Screw without Model or Model without Screw.*



To Illustrate Mr. R. Zimmermann's Paper on a M

FIG. 1. CURVE OF BILGE DIAGONAL COEFFICIENTS.

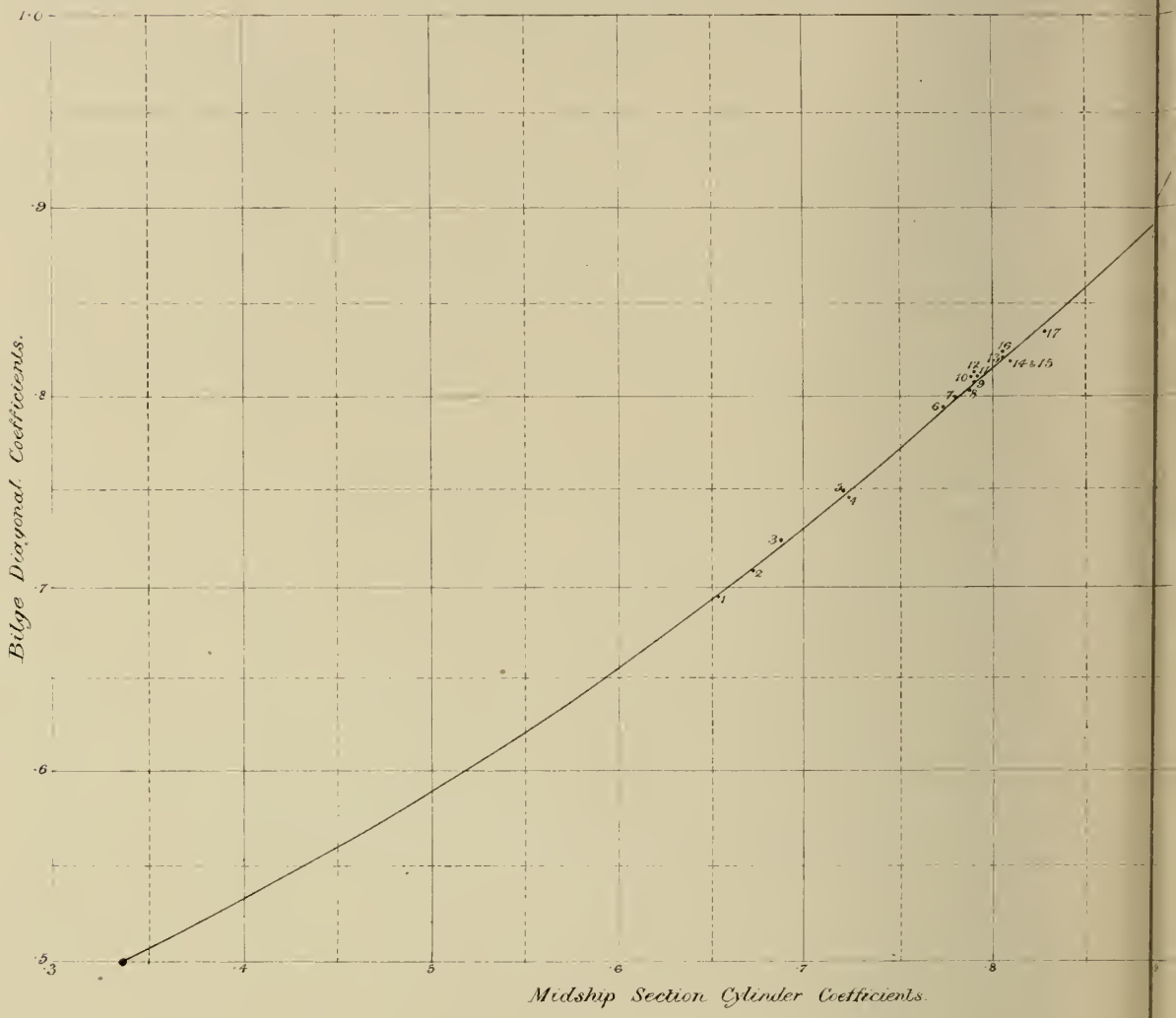
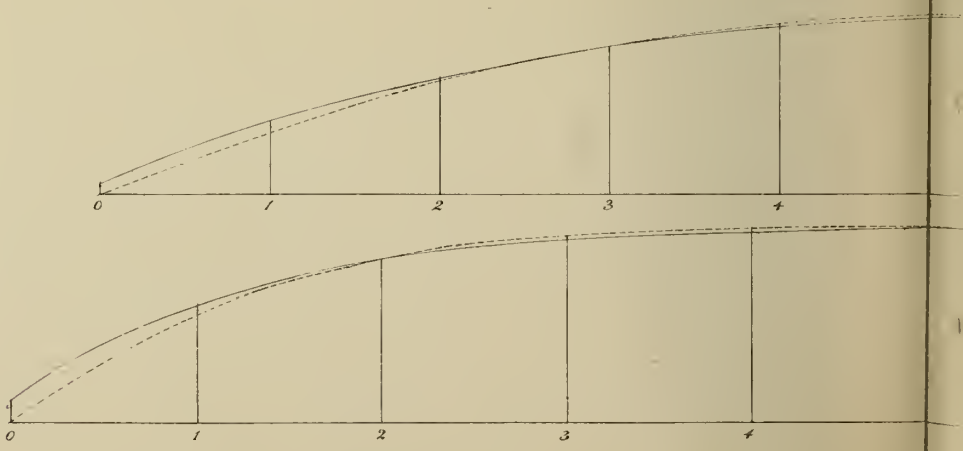


FIG. 3.



'hod of Obtaining the Desired Displacement in Designing Ships

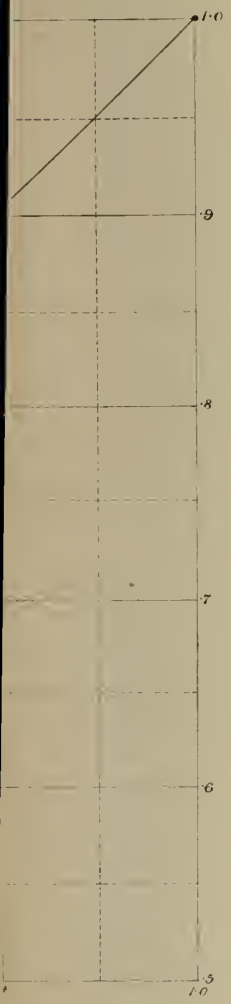
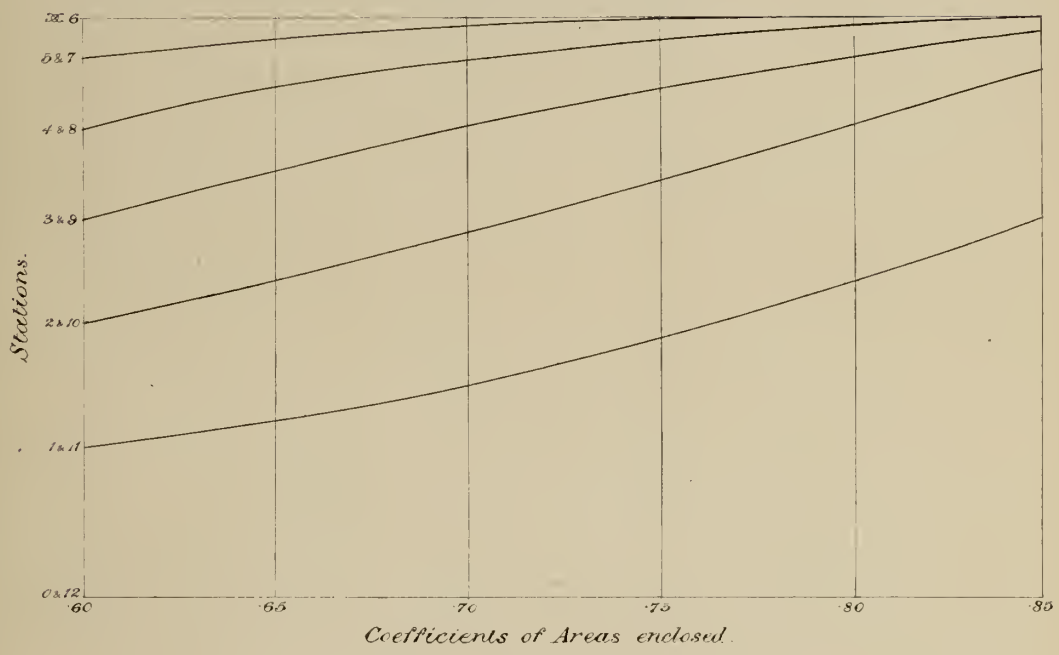
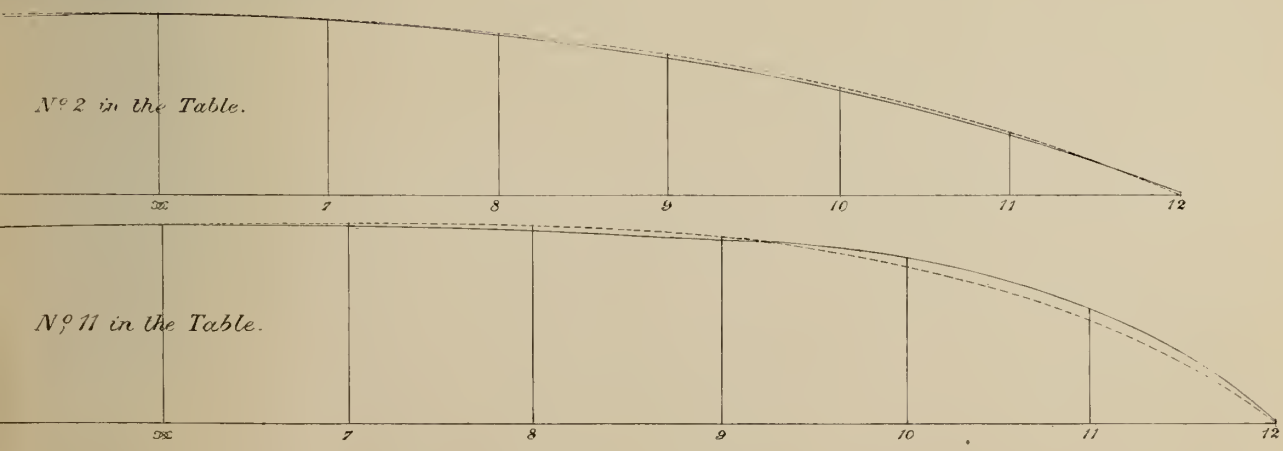


FIG. 2. ORDINATES OF PARABOLAS.



LCE DIAGONAL & PARABOLA.



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